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INTERNATIONAL CO-OPERATION IN SOLAR RESEARCH.

MINUTES OF THE MEETING OF DELEGATES TO THE CONFERENCE ON SOLAR RESEARCH, HELD IN THE HALL OF CONGRESSES, ST. LOUIS, SEPTEMBER 23, 1904.

As CHAIRMAN of the Committee on Solar Research of the National Academy of Sciences, which had issued the call for the Conference, Professor George E. Hale called the meeting to order and explained the purpose of the Conference. After referring to previous movements to secure co-operation in solar research, he emphasized the importance of encouraging individual initiative, and urged that no less attention be paid to such encouragement than to the accomplishment of large pieces of routine work through co-operative effort.[1]

The following officers were then elected to serve for this meeting:

President—Professor George E. Hale.
Vice-President—Professor Henri Poincaré.
Recording Secretary—Professor C. D. Perrine.

A motion that for this meeting the voting be by societies, and that each society be allowed one vote, was adopted.

A motion was made and carried that the chairman of each society's committee should cast the vote for that society.

After some remarks by Professor Hale on co-operation, the following motion was made by Professor Turner:

[1] See ASTROPHYSICAL JOURNAL, December 1904.

That this meeting is in favor of the organization of a scheme of international co-operation in solar research which shall encourage individual initiative, provide suggestions for definite lines of work, and facilitate the collection of results for publication.

This expression, after remarks by Professors Poincaré, Boltzmann, and Hale, was made the sense of the meeting.

Professor Hale stated that in appointing its Committee on Solar Research the National Academy of Sciences had given the committee authority to join the president and foreign secretary of the Academy in presenting its plans for co-operation to the International Association of Academies. The opportunity now existed of securing the views of the Conference on this subject.

After remarks by various delegates upon the relations of scientific societies among themselves and to the International Association of Academies, the following motion was made by Professor Poincaré:

That the committee to be appointed at this meeting negotiate with the Academy of Sciences of Vienna with the view of obtaining the approval and the patronage of the International Association of Academies.

This motion was seconded by Professor Boltzmann and adopted.

A short discussion then ensued upon the fulness of the representation of interested individuals and societies in the preparation of plans for co-operation in solar research.

Dr. Bauer moved:

That the subcommittee on Terrestrial Magnetism and Atmospheric Electricity of the International Meteorological Committee be invited to appoint a committee to co-operate with the Solar Research Committee.

This motion was seconded by Mr. Rotch and adopted.

Professor Turner moved:

That the Hungarian Academy of Sciences of Budapest also be invited to appoint a committee to co-operate with the Solar Research Committee.

This motion was seconded by Professor Frost and adopted.

The following general motion was then made by Professor Poincaré and adopted:

That the International Committee on Solar Research, to be appointed, be authorized to invite, at its discretion, societies and individuals which have been omitted, to co-operate.

The subject of the formation of an international committee was then discussed.

Professor Turner moved:

That each committee participating be invited to appoint a member to serve on the International Committee.

This motion was seconded by Professor Campbell and adopted.

Mr. Rotch, delegate of the subcommittee of the International Meteorological Committee on the relationship between meteorological and solar phenomena, reported on the recent meeting of the subcommittee at Cambridge. At that meeting the invitation of the National Academy of Sciences to co-operate with the other committees was accepted.

A short discussion followed on a provisional program of observations, at the conclusion of which Professor Turner nominated the following gentlemen as a committee to prepare a provisional program, such committee to have power to add to its number at discretion: Professor George E. Hale, Professor Arthur Schuster, Professor Svante Arrhenius.

Suggestions from M. Deslandres as to observations to be undertaken, and the adoption of certain names of instruments and solar phenomena, were, at the suggestion of Professor Poincaré, referred to the Committee on Program.

Following a discussion of routine measurements and computation, Professor Turner offered, on behalf of the Oxford University Observatory, to undertake a portion of this work.

A motion was therefore made by Mr. Abbot that the International Committee enter into negotiations with the Oxford University Observatory with the view of providing for the measurement of plates and the reduction of solar observations.

This motion was seconded by Professor Campbell and adopted.

The suggestion was made by Dr. Bauer that if visual magnetic observations, especially magnetic observations at the eclipse of 1905, could be made at observatories with special instruments provided for the purpose, it was practically certain that the reduction of such observations would be undertaken by the Carnegie Institution.

A memoir on standards of wave-length, prepared by MM. Pérot and Fabry on behalf of the Physical Society of France, was presented by Professor Poincaré.[1]

A motion that standards of wave-length should be determined

[1] Printed elsewhere in this number.—EDS.

from terrestrial instead of solar sources was referred for consideration to the International Committee.

Professor Crew discussed the question of standards of wavelength, and pointed out the necessity of replacing Rowland's standards by a new system. He also presented a paper on this subject by Professor Kayser and a letter from Professor Michelson.

Mr. Jewell presented in abstract a paper on Rowland's standards.

Following is a list of committees and delegates appointed by various societies. Other societies will appoint committees in the near future:

Society	Committee	Delegates Present at Conference
Royal Society of England	Huggins, Christie, Lockyer, Schuster, Newall	Turner
Royal Astronomical Society	Turner, and others to be appointed	Turner
Astronomical Society of France	Deslandres, de la Baume Pluvinel, Guillaume, and others not yet named	Poincaré
French Physical Society	Poincaré, Fabry, Pérot, Deslandres, and others not yet named	Poincaré
German Physical Society	Ebert, Kayser, Kreusler, Lummer, Pringsheim, Runge, Straubel, Wilsing	
Amsterdam Academy of Sciences	Kapteyn, Julius	Kapteyn
Society of Italian Spectroscopists	Tacchini, Riccó, Chistoni, Mascari	
Vienna Academy of Sciences	Hann, Weiss, von Lang, Exner	Boltzmann
St. Petersburg Academy of Sciences	Backlund, and others not yet named	Backlund
Subcommittee of International Meteorological Committee		Rotch
Stockholm Academy of Sciences		Arrhenius[1]
Astronomical and Astrophysical Society of America	Frost, Abbot, Bauer, Jewell, Perrine	Frost, Abbot, Bauer, Jewell Perrine
American Physical Society	Ames, Crew, Lewis, Mendenhall, E. F. Nichols	Crew, Mendenhall
National Academy of Sciences	Hale, Campbell, Langley, Michelson, Young	Hale, Campbell

[1] Acting informally.

A motion by Professor Poincaré was then adopted referring these communications to the International Committee.

Owing to lack of time for discussion, the subject of total solar eclipses was referred to the International Committee, upon motion of Professor Campbell.

Oxford and Meudon were suggested as places for the next meeting. The International Committee was empowered to decide both the place and time of meeting, on motion of Professor Frost, seconded by Professor Poincaré.

Upon motion of Professor Poincaré, the thanks of the Conference were tendered to Professor Hale for his interest and activity in connection with the Conference.

The Conference then adjourned.

GEORGE E. HALE,
President.

C. D. PERRINE,
Recording Secretary.

CO-OPERATION IN SOLAR RESEARCH.[1]

By George E. Hale.

We have come together, gentlemen, in response to an invitation from a committee of the National Academy of Sciences to consider the possibility of inaugurating a plan of international co-operation in solar research. Such a movement cannot be considered an entirely new one; for in the history of astrophysics we find records of several undertakings of a similar nature, some of which have yielded results of great importance. Foremost among these, perhaps, should be classed the important work of the Society of Italian Spectroscopists, which, through the observations of its own members and the effect of their example upon others, has added in a high degree to our knowledge of the Sun. We should also remember the important work inaugurated by Wolf and ably continued by his successor, Wolfer, at Zurich, of compiling Sun-spot numbers from observations made in all parts of the world. The well-known series of solar photographs discussed at Greenwich is another instance of a similar kind; for the negatives are secured through the co-operation of the Royal Observatory at Greenwich, the Solar Physics Observatory at Dehra Dun, in India, and the Observatory at Mauritius. These photographs constitute the most important record of Sun-spot statistics now available. Indeed, they are so complete as to render similar work unnecessary, so far as the duplication of the extensive reductions go, on the part of other observatories. The initiation of this series of photographs is due to the Solar Physics Committee appointed by the British government in 1879. In 1885 this committee proposed to hold an international conference in London to consider plans for co-operation in solar research; but through the inability of delegates to attend the conference the project seems to have been abandoned. At the congress held in Paris, in 1887, to prepare plans for making the International Photographic Chart of the heavens, there was some discussion of inaugurating co-operative investigations of an astro-

[1] Introductory remarks of the chairman of the Committee on Solar Research of the National Academy of Sciences.

physical nature; but hitherto they have not been carried into effect. More recently, a solar commission appointed by the Astronomical Society of France recommended that a general plan of solar research be undertaken. So far as I am aware, however, the work of this solar commission has hitherto been confined to France. At the recent meeting of the British Association in Cambridge a subcommittee appointed by the International Meteorological Committee met to consider "the combination and discussion of meteorological observations from the point of view of their relations with solar physics." The principal object of this movement, as will be seen from the announcement of the purpose of the committee, is to deal with meteorological questions related to solar phenomena. In the consideration of purely solar questions, I am happy to say that this committee will co-operate with the other committees represented at the present conference.

From this brief, and avowedly incomplete, historical sketch it will be seen that the movement we are inaugurating today can lay no claim to novelty. It is merely the culmination of a natural series of events, each of which has tended to emphasize the importance of solar research and the desirability of harmonious co-operation among the investigators at work in this field. I have had frequent occasion to point out the great possibilities which lie open to the observer of solar phenomena. It may be said without exaggeration that no department of science, so far as I am able to judge, offers more promising opportunities for important developments. In almost all cases solar observations are now being made with the methods and apparatus of a quarter of a century ago, and the powerful instruments of modern times, both astronomical and physical, have been but little applied in solar research. I do not mean that this is a rule without exceptions, but it nevertheless applies in a large proportion of cases. There can be no question that great advances in our knowledge of the Sun merely await the application of instruments and methods already available, but hitherto rarely employed.

So far I have spoken with special reference to observations of solar phenomena, but there are other questions of general spectroscopy which should play a no less important part in the deliberations of this conference. No one who is acquainted with the progress of

spectroscopy during the last twenty-five years in the physical laboratory can deny that its methods and its problems should be considered at the same time with the problems of solar physics. Within a very short time laboratory experiments on the phenomena of anomalous dispersion have yielded results so striking as to lead certain investigators to interpret solar observations on an entirely new basis. Although most solar physicists will doubtless hesitate to accept the radical changes in solar theory which have been advanced, they must nevertheless be prepared to give careful consideration to the new claims. I might continue by pointing to many other phenomena of the laboratory, which must be studied in the closest union with solar questions; but this is needless before the present company. One question of the greatest importance—that of standard wave-lengths—will be fully treated by Professor Crew, and therefore need not be discussed by me. In my opinion, if I may be permitted to express it, the discussion of this and other similar subjects at the present conference should be regarded as preliminary, subject to further consideration by the various committees before final action is taken.

Two fundamentally different views are entertained among astronomers regarding the desirability of inaugurating schemes of co-operation in research. On the one hand, there are those who strongly favor co-operative undertakings, on the ground that through the intelligent distribution of work, which such a scheme assumes, large investigations can be carried out with the minimum expenditure of time and effort. Many who hold this view frankly assume that the participants in a co-operative undertaking are to perform their respective parts like so many machines, following a hard-and-fast program from which no deviation can be permitted. This system involves the existence of a central bureau which exercises control over all of the work and, in some cases, carries out the extensive reductions incidental to the research. This view is opposed by a large number of investigators, who think that the future of science depends in far greater degree upon the development of new ideas and the encouragement of individual genius than upon the accomplishment of any piece of work, no matter how extensive it may be. They urge, and with strong reason, that those who engage in a co-operative undertaking, the lines of which have been sharply defined in advance, leaving no

option to individual participants, must inevitably suffer, in so far as their originality of thought is concerned. Thus, while admitting that important results, perhaps not otherwise obtainable, can be secured by co-operation among individuals and institutions, they maintain that the loss will be greater than the gain in case the effect of the domination of some central authority is such as to stifle individual effort and prevent the development of new ideas and methods.

During the last few years these two phases of the subject have received much discussion in connection with many co-operative undertakings in science, variously organized and developed, and playing an increasingly important part in modern research. In spite of objections urged by some of the ablest investigators, these co-operative undertakings are actually being carried out; some with marked success, and others with less promise as to their future outcome. It must not be forgotten that in different departments of science the nature of the work varies so greatly as to make the question of co-operation apply in very different ways in different cases. In meteorology, for example, it is obviously essential that a great number of institutions should make routine observations at appointed hours and upon a carefully arranged plan, in order that the results may be suitable for systematic reduction and study. The same may be said of many other classes of work, particularly those which involve routine observations of secular phenomena. But it will be recognized that even in such a case there is nothing to preclude the development of new ideas and methods by those who are taking part in such a co-operative investigation. The danger, if any danger exists, is that the individual observers may be allowed to believe that the instruments and methods they are called upon to employ represent the highest possible development, and that any deviation from the scheme must stand in the way of the results it is desired to attain.

In the history of science, instances are not lacking of co-operative undertakings which have dealt injury to participants. Instead of encouraging the development of new ideas, they have allowed the entire stress to be laid upon the accomplishment of given pieces of work according to a strict routine, and, either directly or indirectly, they have certainly discouraged individual effort, and thereby hindered progress. It is with a full appreciation of this fact that I

approach the subject of co-operation in solar research. While I believe unquestionably that a science like our own cannot accomplish the most important advances without the collection of extensive data, beyond the reach of any individual or institution, I nevertheless feel that the future of the science depends in a much higher degree upon the encouragement of individual initiative. In suggesting, with others, the desirability of adopting some general program of solar research, in which the observers and instruments in various parts of the world may find a part, I do so only on the supposition that such a movement need involve no discouragement of individual effort. Indeed, I would say more than this. I am not in favor of entering upon a co-operative undertaking unless it can be so planned as actually to encourage individual effort through the provision of conditions which would otherwise be lacking. Specifically, I believe that one of the principal objects of establishing committees on solar research should be the encouragement of little-known investigators to bring forward and develop ideas that may have originated with them. It may frequently happen that the special atmospheric or instrumental conditions required in the accomplishment of a new piece of work may be readily available at some existing observatory, where they could be placed at the disposal of an investigator who might employ them in the realization of his ideas. Through the work of the various national committees, such cases would easily become known, and, in many instances, important new ideas might thus be advanced which would otherwise remain unknown.

At the very outset of the present undertaking, therefore, I would propose that the encouragement of individual ideas be regarded as one of the most important functions of any organization we may establish. But, while attaching so much weight to this phase of the subject, I see no incompatibility between the encouragement of original ideas and the accomplishment of certain pieces of work which can best be done through co-operation. For example, let us take the observation of the spectra of Sun-spots. No one who has observed the spectrum of a Sun-spot and attempted, map in hand, to identify and record all of the widened lines, is likely to be so ambitious as to desire to include the entire length of the spectrum in his program of daily observations. To secure adequate knowledge

of Sun-spot spectra, it seems to me essential that practically all of the lines affected in the spots should be recorded. Those who share this view would undoubtedly be content to confine their attention to certain limited regions of the spectrum. At the time of the Sun-spot maximum, when many spots must be observed, it is obvious that the extent of the spectrum covered by one observer should not be too great. Furthermore, on account of interruptions from bad weather and the desirability of securing a check on the observations, it is equally obvious that the same region of the spectrum should be observed regularly at more than one institution. Thus our knowledge of the spectra of Sun-spots would undoubtedly be materially increased if an understanding were reached as to the division of the spectrum among different investigators. From my knowledge of the work of this kind now in progress, or contemplated, I would anticipate no difficulty in securing such a division of labor. I can see no reason why a mutual understanding on this subject could be otherwise than advantageous to all who are interested in solar research.

In this reference to an important department of solar physics, which has been sadly neglected up to the present time, I have purposely confined my remarks to pure routine observations, such as are involved in the recording of the widened lines. But in this same field of research there is almost unlimited opportunity for individual investigations, many of which might involve the application of new and original ideas upon the part of those who pursue them. Hitherto, strangely enough, little or no attention has been given to the quantitative side of this subject, and few records of the carefully measured positions of widened lines can be found in the literature of astrophysics. Thus we have no knowledge of any such displacement of lines as may be produced by pressure or by the bodily motion in the line of sight of the gas which gives them rise. Line-distortions, it is true, have sometimes been measured; but my present reference is to the less violent motions which may possibly affect the entire mass of gas within the Sun-spot. There is large opportunity, also, for observations of the fine lines into which Young resolved the spot-band, as well as the interesting reversals so recently recorded by Mr. Mitchell, working under Professor Young's direction at Princeton. What I have said relates simply to visual observations, which must

continue to play a very important part in the study of spot-spectra. There is a great opportunity, however, for the development of photographic methods in this connection, and here, again, there will be abundant opportunity for the individual explorer.

This single instance will serve at once to illustrate the desirability of co-operation in solar work and the possibility of securing a system of co-operation which will accomplish the desired purpose and yet leave the individual free to initiate new classes of work. Numerous similar instances might equally well be mentioned. In all cases the development of the method by those who take part in the work should be encouraged in every possible way.

I have dealt at such length with this question because it is so frequently assumed that co-operative work must injure the individual observer. I believe, on the contrary, that a system can be devised which, through co-operative effort, will result in an increase in the number of original suggestions and a more rapid improvement of observational methods.

REMARKS ON STANDARD WAVE-LENGTHS.[1]

By Henry Crew.

It is a matter of no little interest that the man who laid the foundations of mathematical astronomy and devised the corpuscular theory should also have been the first to give us an absolute measure of the wave-length of light. Newton's values, published in 1704, remained unsurpassed for more than a century.

In 1815, Fraunhofer employed his newly invented gratings to determine some of the principal solar lines; these remained standard for nearly half a century.

Kirchhoff in 1862 gave us his beautiful prismatic map of the solar spectrum, a model of precision and care.

The first normal map of the solar spectrum was published by Ångström in 1868. It was based upon his own determinations of absolute wave-length and was printed on a scale such that one millimeter on the map indicates a difference of one tenth-meter in wave-length. For twenty years this work stood in the front rank. But in the later eighties there began to appear measurements of both solar and metallic spectra which introduced an order of accuracy hitherto undreamed of. Thalén, in 1884, pointed out the numerous sources of error in Ångström's map and measures.

But the year 1888 marks the beginning of a new era. For it was at this date that Rowland put out the final edition of his superb photographic map of the solar spectrum, and it was in the same year that Kayser and Runge gave us their exquisite rendering of the spectrum of the iron arc, both of these maps being based upon the absolute wave-length of the D lines, which Bell had just completed with extraordinary care by the grating method. A short list of solar standards was published by Rowland at the same time.

I remember very distinctly the general feeling in the spectroscopic circle which I then knew that, with the publication of Rowland's map, accompanied by his relative wave-lengths, and Bell's absolute

[1] Introductory remarks on opening the discussion on this topic at the solar conference.

value, we were at last on solid ground where a stand of many years might be made.

On the contrary, in 1893, Michelson had—by a method as yet unimpeached—already translated the standard meter into terms of the red cadmium ray, and had incidentally proved that Bell's value of the absolute wave-length was too large by something like two-tenths of an Ångström unit.

Since then Michelson's values have been confirmed as perfectly as possible—at least, without the introduction of a totally different method—by Fabry and Pérot, and Hamy.

In this same year, 1893, appeared Rowland's "New Table of Standard Wave-lengths," including both metallic and solar lines, upon which Hasselberg has based his highly accurate series of metallic arc spectra, starting with chromium in 1894.

In 1895 Rowland began, with the first number of the ASTROPHYSICAL JOURNAL, the publication of what he modestly called a "Preliminary Table of Solar Spectrum Wave-Lengths," expecting, as he says, "to add to it and correct it for a term of years until I can publish a standard list of the lines of the solar spectrum with all the elements to which they belong." From the very beginning the wide divergence between this table and the more probably correct absolute values of Michelson was admitted. The relative values of the table appeared for the time being almost beyond criticism.

In 1896 Jewell[1] published his observations on the want of exact coincidence between solar and corresponding metallic lines, showing not only that the solar lines are displaced in general toward the red but that the different lines of any one element are *differently displaced*. The effect of this was, using Mr. Jewell's own words, "to make the lines of the solar spectrum step down from the commanding position which they have occupied as standards of reference."

For the next four years matters remained about as Jewell's paper left them.

In 1900, however, Kayser discovered certain inconsistencies in Rowland's 1893 table, that is, the "New Table of Standard Wave-lengths." He found also that the distribution of iron lines in this table was such as to render it quite inadequate for purposes of accurate

[1] ASTROPHYSICAL JOURNAL, **3**, 89, 1896.

interpolation. To use Kayser's own words concerning a certain group of Rowland's iron standards: "My correction curves depending upon these lines always showed a quite impossible bend at λ 3400, so that I was finally compelled to omit these standards."[1] These "impossible bends" in Kayser's curves refer to differences as great as 0.02 tenth-meter. Kayser at the same time published a fuller and revised list of iron standards, extending from λ 2327 to λ 4495, the relative values of which are based upon Rowland's metallic standards and are claimed to have a mean relative error not exceeding 0.003 tenth-meter. In other words, the *accidental* errors are much less than those of Rowland's "New Table of (Metallic) Standards."

The state of affairs at the time of Rowland's death (spring of 1901) may perhaps be described by saying that three shadows had fallen across the superb work embodied in his "Preliminary Table;" namely, (1) the wide divergence from the correct absolute values, shown by Michelson; (2) the uncertainty attaching to solar lines, shown by Jewell; and (3) certain inconsistencies in relative values, shown by Kayser. I feel, however, that the figure is misleading; and with your permission I will change it and say that the subject had received illumination from three new sources.

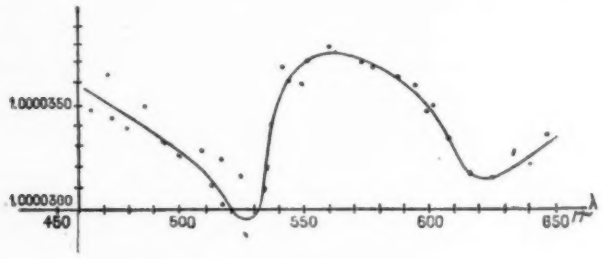
In the year 1902 additional illumination was added from another brilliant source. I refer to the absolute determination of thirty-three solar lines and fourteen iron lines by Pérot and Fabry, using the method of interference. These measures carried with them two important results; namely, (1) they proved that the relative values of the "Preliminary Table" were accurate, over small ranges, to one part in a million; in other words, the curve of errors in this table is a "smooth one;" (2) they proved that the relative values in this table deviate in each direction from the mean axis by as much as 0.02 tenth-meter, that is, by four parts in a million. In short, they proved that the absolute errors in the "Preliminary Table" extend to the first decimal place, the relative errors over any considerable range extend to the second decimal place, while neighboring lines are affected with relative errors only in the third decimal place. The serious consequences of this discovery are realized only when we recall that for seventeen years past all the spectroscopic work of the world

[1] ASTROPHYSICAL JOURNAL, **13**, 331, 1901.

has been based upon this system which the two French physicists have shown to be affected with large systematic errors. In line-of-sight work, where 0.01 tenth-meter means 0.7 km per second, the importance of these errors is enormous.

The upshot of the whole matter is that Rowland's relative values are too small from about the F line to the middle of the green, while from the middle of the green to the D lines his values are too large by about the same amount, namely, an error ranging up to 0.02 Ångström unit.

The year 1902 thus brings us to the end of what may be called



indisputable ground, which may be summarized by saying that Rowland's system, possessing these various defects, is still an instrument of great value, though less perfect than we formerly thought. Just how these errors have crept into the system is a matter of no little importance; indeed, it is a matter of very great importance, if we are to avoid similar errors in the future; at the same time, this is not the principal subject under discussion by this conference. Hartmann has set forth[1] in a clear way the various sources of systematic and accidental errors in Rowland's tables. Michelson has predicted, and Kayser has verified, the fact that for gratings in general, the method of coincidences is not reliable even for relative values, except within limits which are much larger than was formerly believed. But the main question before us is not how the imperfections of the system arose; it is rather how the system may be perfected, with a minimum expenditure of time and energy, so as to answer the pur-

[1] "A Revision of Rowland's System of Wave-Lengths," ASTROPHYSICAL JOURNAL, **18**, 167–190, October 1903. Reprints of this article were sent by Professor Hartmann for distribution among those participating at the solar conference, as containing the expression of his views on the subject.

poses of modern spectroscopy. Various suggestions looking toward this result have recently appeared. These would perhaps naturally form the subject of our discussion. So far as I am aware, the principal alternatives may be summarized as follows in terms of the accompanying correction curve, which Fabry and Pérot published in the ASTROPHYSICAL JOURNAL, **15,** 272, 1902:

1. Correct to the axis of λ as rapidly as determinations of the grade of those by Michelson, and Pérot and Fabry will allow.

2. Reduce to a line drawn *parallel to the axis* of λ at such a distance that the sum of Rowland's errors will be zero.

3. Reduce to a straight line *intersecting the axis* of λ at such an angle as to make the sum of the squares of Rowland's errors a minimum.

These alternatives leave untouched the question of extending the corrections to the ultra-violet region.

RAPPORT SUR LA NÉCESSITÉ D'ÉTABLIR UN NOUVEAU SYSTÈME DE LONGUEURS D'ONDE ÉTALONS.[1]

Par A. PÉROT et CH. FABRY.

LA SOCIÉTÉ FRANÇAISE DE PHYSIQUE, comme suite à la communication de la National Academy, a nommé une commission chargée d'étudier la question de l'établissement d'une échelle de longueurs d'onde. C'est sur l'invitation de cette commission que nous avons préparé le présent rapport pour le Congrès International de Physique de l'Exposition de Saint Louis.

Nous avons, dans les lignes suivantes, cherché à exposer le plus clairement possible la nécessité de l'établissement d'une échelle de longueurs d'onde, à montrer que le spectre solaire devait être abandonné comme étalon, et à définir l'unité absolue.

La mesure des longueurs d'onde, outre qu'elle est nécessaire aux chimistes pour charactériser les corps par leur spectre d'émission, offre des moyens uniques d'investigation, aux physiciens en particulier pour l'étude de l'émission lumineuse et de la constitution des radiations, aux astronomes pour la mesure des vitesses radiales des astres et la recherche des phénomènes qui se passent à la surface de notre soleil, mais, pour que ces études aient une base certaine, il faut que les longueurs d'onde soient mesurées avec toute la précision voulue, et il est intéressant au point de vue international d'adopter une unité définie qui, une fois choisie, ne devra plus varier.

Dans les recherches spectroscopiques, on est amené généralement à faire des mesures relatives, c'est-à-dire à comparer la longueur d'onde de la radiation étudiée à celle d'une radiation connue; s'il est possible avec certaines méthodes, telle la méthode interférentielle, de comparer directement sans aucun intermédiaire les longueurs d'onde de deux radiations quelconques, la méthode employée d'une manière courante, qu'elle repose soit sur l'emploi de prismes, soit sur celui de réseaux, ne permet que la détermination du rapport des longueurs d'onde de raies relativement voisines. Il résulte de là que la détermination d'une seule longueur d'onde serait insuffisante

[1] Présenté au nom de la Société Française de Physique.

pour les applications, et qu'il faut, si l'on veut faire œuvre utile, mettre entre les mains des spectroscopistes les valeurs d'une série de longueurs d'onde convenablement choisies, déterminées par des méthodes instituées spécialement à cet effet avec toute la précision requise. Il en est de même dans d'autres travaux, tels par exemple que le nivellement du sol: loin de rapporter l'altitude d'une station quelconque directement au niveau de la mer, on relie usuellement l'altitude de cette station à celle d'une autre station déjà connue qui sert de base auxiliaire. On voit donc que, s'il est indispensable de déterminer les sources étalons de manière que les radiations émises puissent toujours être produites identiques à elles-mêmes, il est nécessaire également de fixer les valeurs des longueurs d'onde d'un nombre suffisant de points de repère convenablement choisis.

L'établissement d'un exact système de longueurs d'onde est d'une importance capitale, puisque toute erreur sur ces longueurs d'onde fondamentales se reportera dans toutes les mesures spectroscopiques. Il serait désirable que les erreurs contenues dans le système fondamental fussent inférieures ou au plus égales aux erreurs accidentelles qui peuvent résulter des mesures par comparaison. Or il est à peu près certain qu'il n'en est pas actuellement ainsi: les mesures par comparaison peuvent être faites, avec les puissants appareils dispersifs dont on dispose actuellement, avec une précision qui dépasse le millionième; telle serait la précision des mesures si les valeurs des longueurs d'onde des raies de comparaison avaient la même précision: or celle de l'échelle adoptée est beaucoup moindre; aussi, M. Kayser pouvait-il récemment émettre l'opinion qu'une grande partie des désaccords entre les mesures faites par divers observateurs sur une même raie provenait de l'emploi de valeurs incorrectes pour les longueurs d'onde des raies de comparaison.[1]

Depuis les admirables travaux de Rowland, c'est-à-dire depuis environ 15 ans, tous les travaux de spectroscopie ont été faits en prenant comme point de départ les nombres donnés par ce savant. On pouvait cependant déjà prévoir une difficulté dans l'application

[1] H. KAYSER, ASTROPHYSICAL JOURNAL, **19**, 158, 1904: "With a correct system of standards we could now determine the wave-lengths of all the sharp lines—and 99 per cent. of all the lines can be got sharp—with an accuracy of a few thousandths of an Ångström unit. I am sure that the much larger differences found by different observers are caused very often by the use of different, relatively incorrect, standards."

de ce système de longueurs d'onde: les mesures de Rowland ont été faites sur le spectre solaire, il faudrait donc, pour opérer d'une manière correcte, prendre le spectre solaire comme spectre de comparaison, c'est ce que peut-être, aucun observateur n'a fait; on s'est servi de spectres de métaux, en admettant que les longueurs d'onde sont les mêmes que dans le spectre solaire; or l'exactitude de ce postulat est de plus en plus improbable. Il est vrai que certaines raies métalliques, produites par l'arc électrique ont été spécialement mesurées par Rowland, mais ces mesures ne méritent pas la même confiance que celles des raies du spectre solaire; Rowland était convaincu de l'identité absolue des longueurs d'onde dans le spectre solaire et dans celui de l'arc électrique, et lorsque des écarts se manifestaient entre les deux espèces de spectres, il les attribuait à un déplacement de la plaque photographique ou à un défaut de réglage des faisceaux, et il cherchait par des corrections convenables à faire disparaître les écarts dans les résultats.[1] En employant les nombres de Rowland pour des raies de métaux dans l'arc électrique, on emploie donc ou bien des nombres obtenus en mesurant autre chose que ce que l'on emploie, ou bien des nombres un peu incertains; il faudrait se résoudre, si l'on voulait profiter le mieux possible des travaux de Rowland, à employer uniquement le spectre solaire comme spectre de comparaison.

Mais les valeurs données par Rowland pour les longueurs d'onde du spectre solaire sont-elles du moins parfaitement correctes? L'auteur pensait qu'elles étaient exactes au millionième environ. La meilleure vérification consistait à reprendre les mesures d'un certain nombre de raies du spectre solaire lui-même, par une méthode aussi différente que possible de celle de Rowland, et autant que possible plus directe. C'est ce que nous avons fait[2] en comparant directement, par une

[1] ROWLAND, *Physical Papers*, p. 564: "In every plate having a solar and metallic spectrum upon it there is often—indeed always—a slight displacement. This is due either to some slight displacement of the apparatus in changing from one spectrum to the other, or to the fact that the solar and the electric light pass through the slit and fall on the grating differently. In all cases an attempt was made to eliminate it by exposing on the solar spectrum, both before and after the arc, but there still remained a displacement of 1/100 to 2/100 division of Ångström, which was determined and corrected for by measuring the difference between the metallic and coinciding solar lines, selecting a great number of them, if possible."

Voir aussi: L. E. JEWELL, ASTROPHYSICAL JOURNAL, **3**, 89, 1896.

[2] CH. FABRY et A. PÉROT, "Mesures de longueurs d'onde en valeur absolue; spectre solaire et spectre du fer," *Annales de Chimie et de Physique*, Janvier 1902; et ASTROPHYSICAL JOURNAL, **15**, 73, 261, 1902.

méthode interférentielle, un certain nombre de raies du spectre solaire avec une même raie du cadmium. Nos mesures ont porté sur la partie du spectre comprise entre les longueurs d'onde 464 et 647.

La comparaison de nos résultats avec ceux de Rowland conduit aux résultats suivants:

1. Il n'existe pas, dans les tables de Rowland, d'erreurs accidentelles atteignant le millionième; il est même probable qu'au point de vue des erreurs accidentelles, les nombres de Rowland sont au moins aussi précis que les nôtres. Si l'on prend dans les tables de Rowland deux radiations très voisines, les rapports des nombres donnés pour les longueurs d'onde sont toujours parfaitement exacts.

2. Il existe dans les mêmes tables des erreurs systématiques notables (atteignant presque le cent-millième); ces erreurs varient d'une façon régulière en fonction de la longueur d'onde. Si donc on prend dans la table de Rowland les longueurs d'onde de deux radiations situées d'une manière quelconque, le rapport des deux nombres peut être erroné presque de 1 cent-millième, c'est-à-dire d'une quantité dix fois plus grande que les erreurs que l'on peut commettre dans les mesures de comparaison.

L'exactitude de nos résultats a donné lieu au début, à quelques contestations, elle semble aujourd'hui généralement admise.[1] On ne voit d'ailleurs pas comment des erreurs systématiques, fonctions de la longueur d'onde, auraient pu s'introduire dans nos mesures.

Il y aurait donc lieu de reprendre les mesures de Rowland. Les erreurs n'étant que systématiques, et régulièrement variables avec la longueur d'onde, on pourrait se proposer de construire une table

[1] Voir à ce sujet:

LOUIS BELL, "On the Discrepancy between Grating and Interference Measurements," ASTROPHYSICAL JOURNAL, **15**, 157, 1902.

A. PÉROT AND CH. FABRY, "A Reply to the Recent Article by Louis Bell," *ibid.*, **16**, 36, 1902.

LOUIS BELL, "The Perot-Fabry Corrections of Rowland's Wave-Lengths," *ibid.*, **18**, 191, 1903.

FABRY ET PÉROT, "On the Corrections to Rowland's Wave-Lengths," *ibid.*, **19**, 119, 1904.

G. EBERHARD, "Systematic Errors in the Wave-Lengths of the Lines of Rowland's Solar Spectrum," *ibid.*, **17**, 141, 1903.

J. HARTMANN, "A Revision of Rowland's System of Wave-Lengths," *ibid.*, **18**, 167, 1903.

H. KAYSER, "On Standards of Wave-Lengths," *ibid.*, **19**, 157, 1904.

de correction, analogue à celle que nous avons donnée, mais plus étendue, de manière à profiter complètement du travail de Rowland; mais il ne faudra pas, dans ce cas, perdre de vue que les nombres de Rowland ne s'appliquent qu'au spectre solaire, et si l'on veut avoir les longueurs d'onde de raies métalliques, il faudra les comparer directement et sans idée préconçue aux raies solaires. On peut se demander s'il ne serait pas plus sûr, et presque aussi simple, de reprendre complètement le travail de la détermination des étalons. Nous allons donc envisager les diverses solutions possibles, en supposant le travail repris par la base.

Choix des radiations.—La première question à résoudre paraît être celle-ci: Convient-il, pour définir l'échelle des longueurs d'onde, d'adopter des radiations empruntées au spectre solaire, ou des radiations d'origine artificielle?

C'est la première de ces solutions que l'on a adoptée au début de la spectroscopie (Ångström), et que Rowland a conservée. Il est certain que l'emploi du spectre solaire a l'avantage de dispenser l'observateur de tout soin à donner aux sources de lumière, et que le nombre immense des raies de son spectre offre dans certains cas des avantages; ce grand nombre de raies était à peu près nécessaire pour l'emploi de la méthode des coincidences de Rowland. Mais l'emploi de ce spectre offre des inconvénients qui contrebalancent, et bien au delà, ces avantages: en dehors des altérations régulières de longueur d'onde, produites par la rotation du soleil et par les mouvements relatifs réguliers de la terre et du soleil, altérations dont on tient facilement compte, la chromosphère solaire est le siège de mouvements violents; d'autres causes mal connues peuvent agir, et l'on a des exemples de changements, momentanés il est vrai, mais extrêmement importants de ce spectre.[1] Si les nouvelles théories solaires de M. Julius[2] se confirmaient, de petites variations de longueurs d'onde

[1] THOLLON, *Annales de l'Observatoire de Nice;* HALE, ASTROPHYSICAL JOURNAL, **16,** 220, 1902.

[2] W. H. JULIUS, "Solar Phenomena, Considered in Connection with Anomalous Dispersion of Light," ASTROPHYSICAL JOURNAL, **12,** 185, 1900.

"Peculiarities and Changes of Fraunhofer Lines Interpreted as Consequences of Anomalous Dispersion of Sunlight in the Corona," *ibid.*, **18,** 50, 1903.

"Les théories solaires et la dispersion anomale," *Revue générale des Sciences,* **15,** 480, 1904.

des raies solaires n'auraient plus rien de surprenant. En outre comme nous l'avons fait déjà remarquer, le spectre solaire n'est jamais employé comme spectre de comparaison, pas plus par les astronomes que par les physiciens; on se sert toujours de radiations artificielles, qui ne peuvent être que des étalons secondaires si le spectre solaire est pris comme spectre fondamental. L'emploi de raies sombres pour la définition d'une longueur d'onde est peut-être moins simple et moins avantageux que l'emploi de raies brillantes. Enfin, la mesure de la longueur d'onde d'une raie sombre par les méthodes interférentielles, qui seront sans doute employées dorénavant pour les mesures fondamentales, est beaucoup moins facile et un peu moins précise que la mesure d'une raie brillante.

Si l'on se décide à abandonner le spectre solaire pour l'établissement de l'échelle fondamentale des longueurs d'onde, on est forcément amené à prendre des sources de lumière artificielles donnant des raies brillantes (gaz rendu lumineux par des procédés convenables).

Les radiations choisies doivent satisfaire aux deux conditions fondamentales suivantes:

1. Il faut se mettre à l'abri de toute variation possible de la longueur d'onde, et pour cela définir exactement toutes les circonstances qui définissent l'état du gaz et la manière dont il est rendu lumineux. Cela exige certainement, quelques précautions, et sur certains points de nouvelles études sont désirables; mais on peut affirmer dès maintenant qu'il est possible d'avoir une constance absolue de certaines longueurs d'onde. En tout cas l'emploi de sources artificielles présente sur l'emploi de la lumière solaire cet avantage immense que l'on peut expérimenter sur elles, tandis que sur la lumière solaire, on ne peut qu'observer.

2. Il faut que les raies brillantes employées comme étalons soient suffisamment fines, de manière à définir une longueur d'onde bien déterminée. Il faudra éviter de se servir de raies accompagnées de satellites, à moins qu'ils ne soient très faibles, de telle sorte que la longueur moyenne ne diffère pas de celle de la composante principale, ou qu'ils ne soient suffisamment écartés pour que l'on puisse employer l'une des composantes bien déterminée.

On peut dès à présent affirmir que les spectres de certains métaux dans l'arc électrique donneront un grand nombre de raies satisfaisant à toutes les conditions requises.

Les étalons primaires ainsi choisis pourraient ne pas être extrêmement nombreux; quelques dizaines dans le spectre visible et ultraviolet suffiraient probablement; il serait facile de leur rapporter par interpolation toutes les autres raies que l'on jugerait commode d'employer comme étalons secondaires. Quant au spectre solaire, son étude rentrerait dans le domaine de l'astronomie physique, comme un moyen extrêmement puissant pour l'étude des phénomènes solaires.

Quant à la question de détermination de la longueur d'onde de ces radiations étalons, elle semble *a priori* pouvoir être faite suivant une unité de longueur arbitraire; on pourrait songer à prendre par exemple la longueur d'onde de la raie rouge du cadmium égale à l'unité; le nombre caractérisant une longuer d'onde serait alors le rapport de cette longueur d'onde à la longueur d'onde du cadmium, mais il semble bien préférable d'adopter une unité rattachée directement au système métrique; les comparaisons des longueurs d'onde entre elles comportant, semble-t-il, des erreurs du même ordre que celles qui ont pu être commises dans la mesure de la longueur d'onde de la raie rouge du cadmium faite par MM. Michelson et Benoit; ces dernières erreurs, si elles existent, n'apparaîtraient pas; la mesure en valeur absolue d'une seule longueur d'onde suffit d'ailleurs puisque l'on peut, sans difficulté, par la méthode interférentielle, comparer la longueur d'onde d'une radiation quelconque à celle de la radiation choisie comme étalon primaire. C'est, du reste, ce que nous avons fait dans tous nos travaux sur ce sujet, et une longue pratique nous a montré que l'emploi des raies du cadmium ne présente pas de difficultés, et définit une longueur d'onde avec une précision qu'il sera bien facile de dépasser. Nous avons déjà donné les longueurs d'onde d'un certain nombre de raies du fer comparées de cette manière à la raie fondamentale du cadmium.[1] Tout récemment M. Kayser a préconisé la même solution, et a annoncé que des mesures de ce genre étaient en cours dans son laboratoire.[2]

M. Hartmann a proposé récemment[3] de choisir une unité de longueur telle que les nombres de Rowland soient le moins possible altérés; en d'autres termes, les rapports entre les nombres de Rowland

[1] *Ann. de Chim. et de Phys.*, Janvier 1902, et ASTROPHYSICAL JOURNAL, **15**, 73, 261, 1902.

[2] *Ibid.*, **19**, 157, 1904. [3] *Ibid.*, **18**, 167, 1903.

n'étant pas exacts, on ne peut dire qu'ils soient rapportés à une unité définie quelconque, mais on peut chercher une sorte d'unité moyenne, telle que ces nombres soient altérés le moins possible; M. Hartmann a calculé une table de correction, simple transformation numérique de la nôtre, qui est basée sur cette condition. Il est évident que si, comme nous le proposons, on se décidait à reconstruire de toutes pièces une nouvelle échelle de longueurs d'onde, cette solution n'aurait aucune raison d'être. D'ailleurs pour que l'énorme travail de Rowland ne soit pas perdu (on sait que la *preliminary table* contient environ 20,000 raies), il suffirait de comparer par interpolation quelques raies solaires avec les étalons fondamentaux, et de construire une table de corrections plus étendue et peut-être plus exacte que celle que nous avons donnée.

Influence de l'air.—Les longueurs d'onde doivent être définies par leurs valeurs dans l'air. Les valeurs absolues étant très notablement affectées par les variations de température et de pression, il convient, comme l'ont fait MM. Michelson et Benoit, de définir soigneusement ces conditions pour les mesures absolues; mais les rapports des longueurs d'onde sont très peu affectés par les variations atmosphériques, à cause de la faible dispersion de l'air. Cependant, dans des conditions extrêmes de température et de pression, et pour les extrémités du spectre, les variations des rapports des longueurs d'onde peuvent porter sur les millionièmes. Il y a donc lieu dans la définition de l'échelle des longueurs d'onde de dire à quelle température et à quelle pression elle a été établie, mais une définition assez grossière de ces conditions suffit. Dans les mesures par interpolation, il n'y a jamais à tenir compte des conditions atmosphériques.

La solution qui s'impose consiste donc à adopter comme étalon fondamental une raie du cadmium sous certaines conditions de température et de pression, produite dans des conditions rigoureusement déterminées, connue en valeur absolue, grâce aux mesures de MM. Michelson et Benoit.

Les conclusions du présent rapport sont donc:

I. Il y a lieu d'établir une échelle nouvelle de longueurs d'onde étalons.

II. Ces longueurs d'onde seront celles de radiations dues à des sources artificielles, parfaitement définies, et susceptibles d'être reproduites toujours les mêmes.

III. Elles seront mesurées par des expériences spéciales faites par différents expérimentateurs relativement à la radiation rouge du cadmium, produite par le passage d'un courant alternatif ou d'une décharge de bobine dans un tube de Michelson à électrodes d'aluminium dont le tube capillaire a 1 cm de longueur et 2 mm de diamètre.

IV. Provisoirement, et jusqu'après de nouvelles expériences, la longueur d'onde de la raie rouge du cadmium ainsi définie sera considérée comme égale à 6438.4722, dans l'air à 15° sous la pression de 760 mm de mercure.

V. Parallèlement à ce travail, on déterminera une courbe de corrections relatives aux mesures de Rowland.

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NEW STANDARDS OF WAVE-LENGTH.

By H. KAYSER.

FOR many years spectroscopists had to be content with the determinations made by Ångström for the visible part, and extended later by Cornu to the ultra-violet part as far as λ 3000. For the wave-lengths to λ 2200 Liveing and Dewar and Hartley gave the first measurements. At that time errors between 1 and 5 Ångström units were not infrequent, *i. e.*, wave-lengths could be determined to only about one one-thousandth of their value. An immense advance was made by Rowland. He no longer tried to determine absolutely the wave-lengths of a great number of lines by the grating, but, applying the much more accurate method of coincidences, found the values of some hundred lines between $\lambda\lambda$ 2000 and 7600, relative to a value of the D lines taken somewhat at random from the absolute measurements by Bell, Kurlbaum, Müller and Kempf, Peirce. The method of coincidence, as is well known, is founded on the theory of the concave grating, by which at the same point a line of wave-length λ in the first order, of $\frac{\lambda}{2}$ in the second order, of $\frac{\lambda}{3}$ in the third, and so on, is brought to focus.

Rowland expressed the opinion that none of his standards could deviate by a hundredth of an Ångström unit from the correct value, and this seemed probable when one considered the first-rate instruments used by Rowland, and the eminent ability of Rowland and his assistant Jewell for such work.

This faith received the first shock when Michelson by his beautiful interferometer method determined at Paris, in collaboration with Benoit, the absolute values of three *Cd* lines. Not only the absolute values were greatly different from Rowland's, but the relative values also showed discrepancies amounting to more than 0.03 Ångström units. Rowland's system received a much severer blow, when Professors Pérot and Fabry by another kind of interferometer measured many solar and iron lines in the visible spectrum and showed that Rowland's system contained values alternately too high and too low.

It is now quite certain that Rowland overestimated the correctness of his measurements, and that in using his numbers as standards we never can be sure of obtaining relatively exact numbers within 0.01 unit. Besides these fluctuating errors pointed out by Pérot and Fabry, many of Rowland's lines have individual errors amounting to 1 or 2 hundredths, as anyone may see by using different standards for the determination of the same line. Some years ago I tried to give a table of iron standards free from these individual errors, but of course my table, being based on Rowland's, is subject to nearly the same fluctuations of values.

For the, in practice, more important ultra-violet part of Rowland's system we have no evidence whatever of correctness or incorrectness; but it is highly probable that it will contain errors of the same kind as the visible part.

If one tries to determine the wave-length of a sharp line—and nearly 99 per cent. of all spectrum lines can be obtained sharp—by interpolation between neighboring standards, he will find it not difficult by using a good concave grating and a good measuring engine to get with a single measurement the value within five thousandths of a unit, if he uses the same standards; but using others he may get differences of one or two hundredths. So it appears that the uncertainty of our wave-length determinations is caused by the insufficiency of Rowland's standards.

To say a word on the importance of obtaining wave-lengths as correct as possible, for astronomical purposes, for the identification of elements, for the series relations, etc., seems needless. Thus the necessity of creating a new system of standards is evident, and it seems time to consider the way by which a better system may be produced and the errors committed by Rowland avoided.

One of Rowland's errors was the use of mixed solar and arc lines, which he thought were of identical wave-length. Since that time we know, by the publications of Jewell, Humphreys, and Mohler, that they are different. This cause of error could thus be easily avoided, and it seemed as if by applying the method of coincidences to arc lines only, a correct system could be produced. During the last twelve years I have three times tried to do so, but have always failed—I did not know why.

After the publication of Messrs. Pérot and Fabry's measurements of iron lines I formed the plan of founding the method of coincidences on their measurements; it is much easier to get a system of standards throughout the spectrum by using many fundamental lines than by using a single one. But as I was not sure of the absolute correctness to Pérot and Fabry's numbers, though they seemed very reliable, I felt obliged to convince myself. This can be done by the method of coincidences, as one line can be got by that method from others.

While engaged in this research there appeared a paper from Michelson showing that errors in the ruling of gratings are possible which make the method inapplicable: a line of given wave-length in the first order does not coincide with another line of half that wave-length in the second order. I immediately changed the scope of my investigations and made some tests to see if Rowland's gratings have such an error. I have two of Rowland's largest gratings ruled on his second and third dividing engines. The comparison of results obtained with them by the method of coincidences gave differences of more than 0.03 Ångström units, while repeated determinations with each grating alone agreed to a few thousandths.

It is thus apparent that Rowland's gratings are not suited for the application of this method, and it is probable that the larger part of the errors in Rowland's system are due to this cause. As gratings are therefore not to be used for exact absolute determinations, they are useless also for relative measurements, if great accuracy is desired. At present we have no better engines, though different American physicists are planning new constructions, as Michelson and Wadsworth. Time must show if their engines are better. We are now under the deplorable necessity of disregarding the method of coincidences in the creation of a new system of standards; the spectra of gratings can be used only for interpolation between standards not too far apart.

It seems to me, then, that the only way open consists in the determination of a great number of arc lines with some interferometer, and probably Pérot's and Fabry form of that instrument will prove most appropriate. Its use is not easy; it requires long practice even for the visible part of the spectrum, and for the ultra-violet part the difficulties will be much greater. Not a single observer, but several,

should undertake this task, and when their numbers agree to one or two thousandths of a unit for, say, fifty or a hundred lines throughout the spectrum, then the grating may again be applied for getting by interpolation some hundred lines more. Then it will be possible to get all the spectral lines accurate to some thousandths of a unit, a great advance will have been made, and many questions that are now inapproachable can be answered.

Of course, all the measurements of spectra already made—and I have myself worked a good deal in that direction—will be useless as far as wave-lengths are concerned. Professor Hartmann, of Potsdam, thinks it already now possible to calculate from Pérot and Fabry's measurements a table of corrections, by the application of which Rowland's measurements, and all the others based on Rowland's standards, may be corrected. I think that is impossible. We do not even know that the fluctuations in Rowland's system are truly known, as very few lines have been remeasured by Pérot and Fabry, and they may have fallen on some lines with large individual errors, and the curve of corrections may become quite another by the use of other lines. But even if that were not the case, who knows from what individual standards every line of a spectrum has been determined? Hasselberg is the only man who gives in his tables the standards between which he has interpolated, so that his numbers can be corrected when corrections to Rowland's standards are known. The greater errors of individual lines in Rowland's tables cannot be corrected. And, further, it is quite impossible to deduce any table of corrections for the ultra-violet part. As the method of coincidences has broken down for gratings which have been tested, we can get no conclusion whatever as to the behavior of Rowland's system in this part of the spectrum without new absolute measurements.

I therefore think Rowland's system must be entirely abandoned and a new system created. Since for all spectroscopic purposes only relatively correct values are needed, the new system may be based on Rowland's incorrect value for the D lines; then new measurements would coincide within some hundredths instead of tenths of a unit with the old ones. I think it more logical, if an entirely new system is to be created, to base it on Michelson's absolutely exact values of the *Cd* lines. But this question is of minor importance.

SOME TOTAL SOLAR ECLIPSE PROBLEMS.[1]

By C. D. PERRINE.

REAL progress in eclipse work dates from the applications of photography and spectroscopy to the subject. Indeed, the fundamental question whether the corona belongs to the Sun or is a lunar effect was first definitely answered by the sensitive plate.

These methods of investigation, coupled with a fuller realization that the corona is a part of the Sun, and that every fact concerning our Sun is of the utmost importance in the study of the stars in general, brought eclipse problems into great prominence. Following rapidly came the important discoveries of the bright lines in the corona and chromosphere, of the reversing-layer, and of variations in the general form of the corona with the Sun-spot period. Later came the detection of a few Fraunhofer lines in the corona, of the irregular distribution of the "coronium" stratum, of interesting details in the coronal structure, particularly the "hoods" over the prominences, and of the low effective temperature of the corona as measured with the bolometer.

Observations of the last eclipse furnished strong evidence that the corona, Sun-spots, and prominences are closely related phenomena, and that the light of the outer corona is almost wholly reflected or diffracted photospheric light, whereas that of the inner corona is chiefly inherent.

At the 1901 eclipse an unusually favorable opportunity presented itself for determining the motion of coronal matter in a region of abnormal disturbance, from a comparison of photographs secured at different stations. Unfortunately, suitable observations for this purpose were available over only a small interval of time, and the most we can say is that the velocities at right angles to the line of sight were certainly less than 20 miles (32 km) per second. This result points to comparatively low velocities as the normal condition in the corona.

[1] Read at the International Congress of Arts and Science, St. Louis, September 23, 1904.

The question of the existence of intra-Mercurial planets is a distinct eclipse problem. Observations at the last eclipse indicate that there is no such planet as bright as the fifth magnitude, nor any great number as bright as the eighth magnitude.

This, briefly, is our knowledge of eclipse phenomena at the present time—a knowledge which is seen to be general, rather than detailed. The work already accomplished has been of the nature of a reconnaissance, and suggests a concentration on details as the line of future development.

The problem which promises to be solved first, and to be eliminated from eclipse programs, is that of the existence of intra-Mercurial planets. At present there seems little probability that bodies of appreciable size exist and the eclipse of 1905 ought to settle the question so far as objects brighter than the tenth photographic magnitude are concerned. This appears to be a sufficiently faint limit for the present, as a body of tenth magnitude in this region could scarcely be over 12 or 15 miles in diameter. Approximately a million such bodies would be required to explain the observed anomalies in the motion of Mercury. The long-focus cameras used at the last two eclipses proved very efficient in this search. Under good conditions they can record objects of the tenth photographic magnitude, and their power could be readily increased if necessary.

In the event that no planet is found, it will remain for the mathematical astronomer to discover the cause of the discrepancies in Mercury's motion. However, it is not impossible that a ring of very small bodies, numbering many millions, perhaps beyond the power of practicable eclipse instruments, may exist within the orbit of Mercury.

The structure of the corona is one of the most important of eclipse problems and is best studied with long-focus cameras. These should give as large an image as the atmospheric conditions will permit. Cameras of forty feet focus are readily pointed directly at the Sun (a method which has, I think, some advantages), and it is probable that those of 50 or even 60 feet might be so used. Instruments of greater focal length would be advantageous under good atmospheric conditions, but it would be advisable to mount them horizontally.

The entire change in the appearance of the detailed coronal feat-

ures from one eclipse to another shows clearly that the coronal matter is in motion. The appearance of the streamers, and their evident connection in so many instances with prominences and other eruptive phenomena on the Sun's surface, strongly indicate motions away from the Sun. Radiation pressure must also be concerned to a great extent in the outward transportation of the very minute particles of matter. On the other hand, the forms of the hoods and the low order of velocities of coronal matter (indicated by observations at the 1901 eclipse) leave little room for doubt that there is motion inward as well as outward.

It is impossible to overestimate the value of a knowledge of these motions, particularly if motions *in* the line of sight, as well as at *right angles* to it, can be secured. Accurate measures of these motions at several points in the path of a streamer, for example, would permit a satisfactory determination of its form and point of origin to be made. The motions at right angles to the line of sight should be determinable from photographs obtained at intervals of from one to two hours. The 1905 eclipse offers especially favorable opportunities in this respect. Stations may be occupied from Labrador to Egypt. Between these extreme stations there is a difference of two and one-half hours in the times of totality.

The success at the last eclipse in recording the Fraunhofer lines out to a distance of 45′ from the solar limb, through clouds which allowed only a small portion of the light to pass, indicates a possibility of securing some line-of-sight results from the photospheric light in the corona. However, more powerful spectrographs and plates of greater sensitiveness than those heretofore applied would have to be used. Difficulties would arise in the interpretation of such results, for the motion of reflecting and diffracting matter at *right angles* to the line of sight would cause a displacement of the lines as well as the motion *in* that line; but the value of the former component would perhaps be available from the comparison of large scale coronal photographs.

The general character of the light of the corona seems to be pretty conclusively determined. A very small proportion has a bright-line spectrum, which indicates a gaseous nature. The substance emitting the bright-line radiations is thus far unidentified with any

terrestrial elements. The greater part of the light from the inner corona gives a spectrum without any visible trace of the Fraunhofer lines. It is undoubtedly due to incandescence of solid or liquid matter. Practically all of the light of the outer corona gives a spectrum which is an exact counterpart of the solar spectrum. Polarization results are in full accord with this view.

The only evidence which appeared to be at variance with the theory of an incandescent inner corona is the bolometric observation of the Smithsonian party at the 1900 eclipse. This observation showed a small quantity of heat, but very much less than had been anticipated, on the theory of incandescence. The effective coronal temperature observed was but a few degrees higher than the temperature of the observing room. Arrhenius has recently shown that the heat radiation observed at that eclipse is as much as could be expected from incandescent matter under the conditions probably prevailing in the corona. The close agreement of results from such widely differing methods gives considerable confidence in the theory of incandescence.

Theoretically, it is possible, by means of a well-known law, to determine the temperature of the corona from the position of the maximum of intensity in its continuous spectrum. Such a study would be most important, but it may be doubted whether the necessary observations can be secured, from the fact that for the temperatures probably existing in the corona the maxima would fall in the extended region of the spectrum between the green and the infra-red. It would be difficult to observe this region of the spectrum either visually or photographically.

Schuster has shown that with sufficiently accurate polariscopic observations it would be possible to determine the law of distribution of coronal matter, and whether the materials are moving away from, or toward, the Sun. When we consider, however, the variable conditions which are undoubtedly present along almost any line of sight through the corona, it seems practically impossible to obtain trustworthy results except perhaps in the isolated polar streamers of "minimum" coronas.

Photometric determinations of the brightness of the corona, as a whole and of the different features, if systematically carried out, furnish an excellent test for solar and coronal theories. It is still

uncertain whether the "maximum" or "minimum" coronas are brightest. Photometric observations of the coronas of 1893, 1898, and 1900 by the English expeditions indicate a decided increase in brightness [in the ratios 3, 6, and 8 respectively],[1] whereas the large-scale photographs made by the Lick Observatory expeditions upon a systematic plan show a *decrease* for the same eclipses [in the ratios 10, 8, and 5]. [In obtaining the results from the large-scale photographs, the accidental variations due to development, etc., have been eliminated as far as possible by a combination of the results from a number of negatives. The plates used in 1893 were of a lower sensitometer number than those of 1898 and 1900. It seems reasonable to suppose that the later plates are in general more rapid than the earlier ones, the effect of which would be still further to accentuate the decrease in observed brightness. Again, the large-scale photographs of 1893 were taken with the Sun at a lower altitude than at the eclipses of 1898 and 1900; and a correction for this fact would further favor the greater brightness of the 1893 corona.] The desirability of carrying a well-considered program of photometric observations through one or more solar periods is evident.

The wave-lengths of the lines in the spectra of the chromosphere, reversing-layer, and corona should be determined with the greatest possible accuracy in order to make their identification with terrestrial elements more certain. Professor Liveing inclines strongly to the belief that the volatile gases recently discovered in the Earth's atmosphere—argon, krypton, neon, and xenon—are present in the outer strata of the Sun, as observed at eclipses. The decision of this question awaits more accurate determinations of the lines in the flash spectrum.

The use of a moving plate to furnish a continuous record of the spectrum of the Sun's edge is of especial value in obtaining a knowledge of the heights of the substances and of possible changes in the wave-lengths of the lines in their spectra, as their strata are covered and uncovered by the advancing Moon. The fixed plate also has its advantages, in that familiar lines are recorded, but the result is an integrated effect, and changes during the exposure are lost.

A careful study of the depths and distribution of the gaseous

[1] Bracketed portions were not read.

constituents of the corona throughout one or more Sun-spot cycles can scarcely fail to disclose very important facts. The variation in the distribution of "coronium" as observed at different eclipses is an indication which should be followed up closely.

Although the corona is an appendage of the Sun, to what extent it partakes of his surface rotation is questionable. It would, therefore, be of great value to have a determination of rotation for both the gaseous and the non-gaseous portions, from line-of-sight observations. But we must not overlook the difficulties in the way of the solution of this problem. A *difference* in the coronal velocities east and west of the Sun might be detected with moderate dispersive powers such as it may be possible to employ in the not remote future. The determination of the *law* of rotation would require very much more accurate data, however.

The most pressing problem in connection with eclipse work is that of finding a method for observing the corona in full sunshine, thus permitting us to secure a continuous record, as in the case of the prominences. All the methods thus far tried have failed to give positive results.

So far as the gaseous constituent of the corona is concerned, although its quantity is relatively very small, it is theoretically possible to obtain images in its bright lines on the assumption that they are monochromatic. The non-gaseous corona presents much greater difficulties because it is impossible to isolate its radiations by any simple spectrographic method. The attempt to observe the corona in full sunshine by means of the spectrograph using a double-slit gave promise of success, theoretically, which has not yet been realized. It may be pointed out that this method of diffusing the atmospheric glare by taking photographs of the strictly continuous light of the corona through an atmospheric (solar) dark line could only be expected to give an image of the inner corona where the light due to incandescence predominates. Results obtained at the last eclipse indicate that this would not be more than 10 minutes from the limb. The outer corona in which photospheric light predominates would not impress itself through a dark line.

The extremely small quantity of heat radiated by the corona seems to preclude the hope of observing it by temperature methods.

The properties of polarized light enable the contrast between the strongly polarized portions of the corona, and the unpolarized sky illumination to be greatly increased. Experiments at Mount Hamilton, in which the theoretical contrast between corona and sky was increased twelvefold by polariscopic methods, did not reveal the corona to the eye.

There exists at every eclipse the desirability, and at many eclipses the imperative need, of immediate confirmation by different observers. These considerations prompt the thought that too many expeditions with well-considered plans of work cannot engage in eclipse observations, and that an interchange of ideas and discussion of plans in advance is highly desirable. For those problems which require observations extending over many eclipses, or over a number of spot-cycles, the advantages of co-operation are especially great.

LICK OBSERVATORY,
September 1904.

ON A NEW METHOD FOR THE MEASUREMENT OF STELLAR SPECTRA.[1]

By J. HARTMANN.

WHEN Vogel in 1888 introduced the photographic method for determining the radial velocity of stars, he used two coincidence methods in measuring the spectrograms. In his "first method" there is laid upon the plate to be measured a plate of the solar spectrum taken with the same spectrograph. Then the positions of several lines in the stellar spectrum are measured with respect to the corresponding lines in the solar spectrum, and thereupon the displacement of the artificial comparison lines as compared with the corresponding lines of the solar spectrum. The difference of the two displacements then yields the displacement of the stellar spectrum with respect to the terrestrial comparison spectrum. In Vogel's "second method" this latter displacement was measured directly.

The reduction of the measurements is exceedingly simple in the case of this procedure of utilizing coincidences. The desired velocity of the star in kilometers is obtained by simply multiplying the displacement measured with the micrometer screw by a factor which is nearly constant for each wave-length. It was a further great advantage of Vogel's "first method" that the relative position of the stellar and solar spectra was very sharply determined by the use of a very considerable number of lines. The advantage hereby gained was, however, in part lost again by the fact that the displacement of the superposed solar spectrum in respect to the artificial comparison spectrum could be determined by measurements of but a single line. This disadvantage of all coincidence measurements—that they are always restricted to the few lines which are simultaneously present in the comparison spectrum and in the stellar spectrum—appears still more directly in Vogel's "second method." In this process the further assumption is made that the lines in question have precisely the same wave-length in the stellar and in the comparison spectrum—a postulate which certainly is not rigorously fulfilled.

[1] Presented to the International Congress of Arts and Science, St. Louis, September 1904.

It is, therefore, now customary to employ the coincidence process only where a quick and easy determination of provisional results is wanted. For the definitive treatment of the spectrum plates a method is now generally employed which permits the measurement of all lines desired of the stellar spectrum. The reduction of such measurements is then effected by the aid of a dispersion formula which establishes a relation between the wave-length of a line and the reading of the micrometer screw with which the spectrum was measured. I published in the *Astronomische Nachrichten* (**155,** 81–118, 1901) a complete description of this rigorous procedure.

This generally employed reduction process has, however, several quite serious disadvantages, of which I will cite only the two following as of the most consequence. First, the measurement as well as the computation, even in the simplest form that I have given, is still so laborious that it is quite impossible fully to exhaust each spectrum of the class having numerous lines—that is, to measure *all* of the lines by this method. The consequence is that hitherto the observers have always limited themselves to the measurement of a few lines, thus sacrificing the complete utilization of the rich material contained on the plates. It is only in case of spectra of the first type, having few lines, that it has been possible to utilize all of the lines for determining velocity. This has been repeatedly done, so that the measurement of the spectrograms of this sort may be regarded as definitively accomplished.

The second defect in the procedure in question is that the wave-lengths of the lines measured in the comparison spectrum must be assumed to be accurately known. Inasmuch as a change of wave-length of from 0.01 to 0.02 tenth-meters corresponds to a velocity of 1 kilometer, the wave-lengths of all the lines measured would have to be accurately known, at least to 0.01 tenth-meter, if the fractions of a kilometer of the velocity are to be guaranteed. But wave-lengths as accurate as this are available only in isolated instances. Just recently I have pointed out how important it is for astrophysics to have measurements of the wave-lengths carried out referred to a uniform system. Measurements of velocity cannot be definitively reduced so long as these wave-length determinations have not been completed; but still many years will elapse before sufficiently sharp

wave-lengths of all the elements observed in stellar spectra will be available.

But even when these wave-lengths of terrestrial substances are accurately determined, there will still remain the assumption, which surely is only approximately correct, that the lines in question also have precisely the same wave-length in the absorption spectrum of the stars observed.

In order to overcome all these difficulties, and to enable an entirely definitive determination of velocity to be made at present, for all many-lined spectra, I developed several years ago a method which depends upon a wholly new principle of measurement. While in all previous methods the measurement was effected by setting a single thread successively upon the lines of the stellar spectrum and those of the comparison spectrum, the new procedure depends upon the *simultaneous setting of numerous lines of one spectrum on the lines of another spectrum.*

The stellar spectrum is photographed as heretofore, and therefore has on both sides the lines of a terrestrial comparison spectrum. With the same spectrograph a solar spectrum is now photographed in a similar manner and with the same comparison spectrum. The two plates are placed in a specially constructed measuring microscope, and the observer then sees in the field of view, close to and on both sides of the stellar spectrum, the corresponding region of the solar spectrum; and similarly, alongside of the comparison spectrum of the star, that of the Sun. It is then only necessary to move the solar spectrum by the screw so that the lines of the stellar spectrum coincide with those of the solar spectrum, and then so that the lines of the two comparison spectra coincide. The difference of the settings then gives directly the displacement sought.

It is seen that this process requires neither an accurate knowledge of the wave-lengths of the lines used nor any extensive computation of any sort. In fact, it is no longer necessary for the observer to make the settings upon each individual line, for the apparatus is so constructed that long stretches of the adjacent spectra, which in part may consist of irresolvable groups of lines, may always be brought simultaneously into coincidence. The observer is thus enabled to employ *all* the lines of many-lined spectra for measurement; the

measurement is correspondingly more precise, but is, nevertheless, very rapidly executed.

In the reductions, which are as simple as in the case of Vogel's coincidence methods, only the single assumption is made that the lines have the same wave-lengths in the spectra of star and Sun. This assumption is undoubtedly the only permissible one for all stars of the second type, for the treatment of which the process now described was primarily designed. At least this assumption is probably nearer the facts than that hitherto always made, namely, that the lines of these stellar spectra have precisely the same wave-lengths as those in terrestrial substances investigated in the laboratory.

A more precise description of the apparatus for measurement, as well as of the procedure in observing, will be given later.

ASTROPHYSICAL OBSERVATORY, POTSDAM
August 2, 1904.

A DESIDERATUM IN SPECTROLOGY.[1]

By Edwin B. Frost.

It is to be presumed that most astronomers will agree in regarding the knowledge of the stellar evolution as one of the greatest ultimate problems of astrophysics. It can hardly be questioned that great simplification would be gained, and the co-ordination of observations be rendered more certain, if a comprehensive scheme of stellar classification according to spectra could be adopted by all workers in this field. It has seemed to me that the occasion of an International Scientific Congress is particularly appropriate for briefly considering whether a beginning should not soon be made toward developing such a system by the combined efforts of the workers in this department of astrophysics. It is obvious that the common consent of the parties interested is essential to the successful erection of a structure of this sort, and it cannot be the work of any individual, or of any group of individuals which does not fully and widely represent those concerned.

If someone should fear that this is a proposal for the construction of an artificial system, which would only add to the nomenclature and terminology of astrophysics, let us consider for a moment the present status of the question of stellar classification. Differences in stellar spectra were recognized by Fraunhofer. Many years later Rutherfurd published a brief grouping of them. But the classification proposed by Secchi in 1866 has been most widely used, doubtless by reason of the simplicity and obvious distinctness of its four types. Based upon visual observations alone, it must necessarily lack comprehensiveness, and this has led to the suggestion by E. C. Pickering[2] of the addition of a fifth type to include stars showing bright lines.

Vogel's system, logically developed along the line of his pioneer and prescient views of stellar evolution, has been of great service for thirty years. With the increased knowledge acquired from more

[1] Paper read before the Section of Astrophysics of the International Congress of Arts and Science, St. Louis, September 1904.

[2] *A. N.*, **127**, 1, 1891.

extensive and better observational data (in short, from the introduction of spectrography, or the application of photography to the spectroscope), a restatement of the distinguishing features has recently been made, happily by the eminent founder of the system. It now includes ten subdivisions.

In connection with the extensive operations in the field of stellar spectroscopy at Harvard Observatory, a more extended system has been found necessary by Professor Pickering for recording the observed facts, and, in a sense, it may be said that three different classifications have been used in the Harvard publications. The development was carried farthest in the scheme of Miss Maury, which includes more than seventy subdivisions or shadings. Other groupings have been proposed with considerable elaborateness, and as the result of careful study of large quantities of observed data, by Sir Norman Lockyer, and also by Mr. F. McClean. These newer systems have not been proposed by their authors from a desire to associate new classifications with their researches, but from their feelings of the insufficiency of existing systems, or of the lack of adaptability of the existing systems to the purposes of their own work. The systems have all been of service, but the very existence of so many shows quite clearly the need of combined action tending toward the adoption of one of them, or toward their supersession by a new system formed by international co-operation, and representing, as far as possible, the combined views of astrophysicists.

As an illustration of the confusion and mnemonic difficulties of the present classifications,[1] which (excepting Lockyer's of 1899) are merely distinguished by numerals or by letters, let me cite the case of the star *Procyon*. It is assigned by the different authorities as follows:

[1] Literature: SECCHI, *Comptes Rendus*, **63**, 626, 1866; H. C. VOGEL, *Astronomische Nachrichten*, **84**, 113, 1874; *Sitzungsberichte der k. Akad. zu Berlin*, 1895, 947–958; ASTROPHYSICAL JOURNAL, **2**, 333–346, 1895; *Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, **12**, 6–8, 1899; E. C. PICKERING, *Annals of Harvard College Observatory*, **27**, 1890; MISS A. C. MAURY, *ibid.*, **28**, Part I, 1897; MISS A. J. CANNON, *ibid.*, **28**, Part II, 1901; J. N. LOCKYER, *Phil. Trans.*, **184**, 724, 1893; *Proc. R. S.*, **65**, 186, 1899; "Catalogue of 470 of the Brighter Stars Classified According to Their Chemistry at the Solar Physics Observatory, South Kensington," 1902; F. MCCLEAN, "Comparative Photographic Spectra of Stars to the 3½ Magnitude," *Phil. Trans.*, **191**, 127, 1898; "Spectra of Southern Stars," London, 1898.

Secchi, II; Vogel (1875), Ia; Pickering (Draper Catalogue), F; Lockyer (1893), A (β); Vogel (1895), Ia3; Miss Maury, XIIa; McClean Division III; Miss Cannon, F5G; Lockyer (1899), Procyonian. I venture to say that in this company there are not many who could instantly localize a spectrum of Vogel's Class Ia2; or of the Draper Catalogue E; or Miss Maury's VIIc; or of Lockyer's Taurian or Achernian groups.

The lack of defining power of the present numerical or literal groups seems very obvious. It will, perhaps, be felt quite as much by workers in stellar spectrography as by teachers, who cannot fail to keenly realize the student's difficulty in visualizing the spectra described as of "Type 3, Group VI" or Class "M." There is a great advantage in the use, in the classification of a kindred science, of such a term as "carnivora," which is available for all languages and is at once comprehensible.

The field is almost entirely open and untouched, as compared with other branches of science. It is not even agreed—indeed, has it been often considered?—whether there shall be used, in the comparative study of spectra, divisions corresponding to orders, families, and genera. Of the helpfulness of such divisions in other sciences there can be no doubt. Starting thus with the benefit of the experience of the students who use classification in other subjects, it should be possible in this subject to avoid the inconveniences, and even absurdities, which have attached themselves to systems in other branches of science, while utilizing their good points. It is, of course, an essential feature that only such terms should be used as will pass without any considerable change in all of the four principal modern languages.

In petrography, as a result of labors extending over a decade, by five or more American authorities in that field, a scheme of classification has recently been published which is commanding general attention among geologists. Had it been an international undertaking, and more widely representative, it would presumably find very prompt and widespread adoption.

In view of what has been said, it would be quite inappropriate for me, or perhaps for any individual, to advance at this time proposals for the new system. I would merely point out certain questions that

would arise in determining the bases of a classification. It is a distinction of the classifications of Vogel and of Lockyer that they logically correspond to the theories or hypotheses of their authors. They, accordingly, are not in marked agreement; and in the case of any system based upon theories of development, new theories, or an inversion of old theories, would necessarily carry with them still other new classifications. Is it not, therefore, desirable that any new system of classification should be based, rather, purely upon observed data? Theories of today may be reversed tomorrow, and, indeed, what we regard as observed facts may soon receive a diametrically opposite interpretation; nevertheless, no age has a surer basis for its reasoning than what it regards as its observed facts.

In any new classification, simplicity should, of course, be sought, and the subject naturally furnishes obvious distinctions; such are given by bright lines and dark lines, bright and dark bands, spectra having few lines and spectra having many lines, narrow lines and broad lines, bands of different aspects. Then, too, the classification according to chemistry is most natural, and probably reliable. The term "helium" class of spectra would seem unambiguous and definite, although, of course, we realize the presence of helium in the Sun, hence in the stars of the solar type. It is certain that the absence of the lines characteristic of a particular spectrum constitutes no proof of the absence of that element from the celestial body.

In the light of our present knowledge—or, better, in the darkness of our present ignorance—the use of temperature as a basis of classification would seem very doubtful; but, without committal to any theory, the different electrical behavior of the different lines—occurrence of enhanced lines, so called—might properly serve as a criterion in arranging some subdivisions.

It would not seem too much to expect that a new international classification should include at least one hundred separate subdivisions, for of the reality of the distinction between existing groupings there cannot be the slightest question in the minds of those dealing directly with spectrograms. But any new classification should be based upon the broadest facts and the most comprehensive data, already available and to be collected for the purpose in hand. Distinctions must not be based upon comparisons of merely the visual part of the spec-

trum, or of the narrow range in the blue and violet often studied; the whole extent of the ultra-violet attainable should also be included, and studies of the distribution of energy in the entire range of the spectrum should add important data.

In view of the necessary time involved, probably five years, for the fairly complete development of a new system of classification, and in view of the desirability of having the participation of those distinguished pioneers in stellar spectrographic work, yet happily with us, I submit the question: Is it not time that a beginning be made by the organization of an international committee to consider the question of a new classification of stellar spectra, representative of the observable facts of the first decade of the twentieth century?

YERKES OBSERVATORY,
September 15, 1904.

AN ELECTRIC THERMOSTAT.

By HORACE DARWIN.

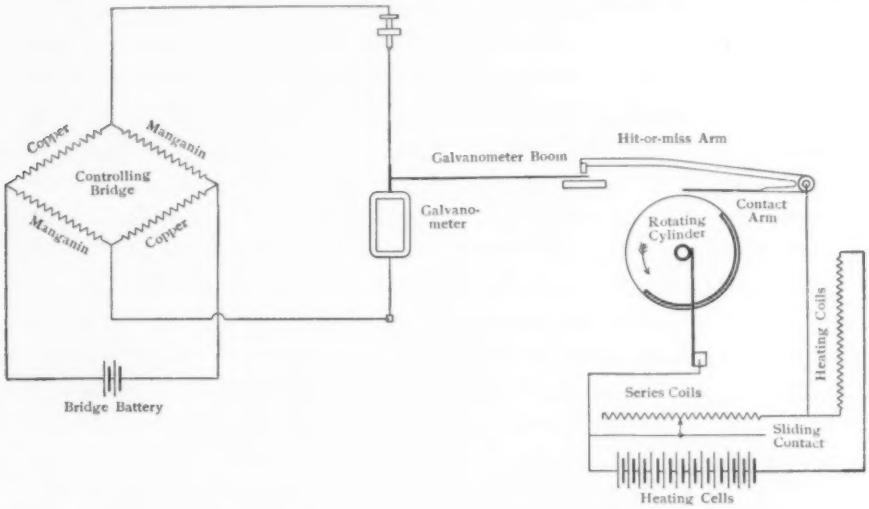
THIS thermostat was designed and constructed by the Cambridge Scientific Instrument Company, Ltd., as an adjunct to the spectrograph of the twenty-four-inch refractor of the Royal Observatory, Cape of Good Hope. The special object in view is to maintain the prisms and other parts as accurately as possible at a known constant temperature, over a considerable period; but, as the same devices are capable of a far more extended application, it appeared that an account of them might be of interest. The prisms are inclosed in a metal box which is surrounded by an outer chamber; the temperature of the air in this chamber is kept constant. The air is rapidly stirred by an electrically driven fan, and the temperature in the box varies very slightly, this slight variation taking place very slowly.

The same device has been used by Lord Berkeley in some delicate experiments he is trying. In this case it is an oil bath which is to be kept at a constant temperature. The oil is rapidly stirred by rotating fans made in the form of screw propellers, and the temperature did not vary by so much as 0°.01 C. for a week at a time.

The heat is supplied by electrically heated coils, and the regulation of the temperature is automatically effected by varying the current passing through these coils. Four resistances, two of copper and two of manganin, are arranged as a Wheatstone bridge, and are placed in the chamber or vessel the temperature of which should not vary. Through this controlling bridge a small and constant current is always passing. One pair of opposite arms are of copper, and the other pair are of manganin, and their resistances are such that they balance at the required temperature, and then no current passes through the galvanometer connected to the bridge. But as the temperature coefficient of the copper arms is large and that of the manganin small, a very small variation of the temperature will throw the galvanometer over to one side or the other. It is this movement of the suspended coil of the galvanometer which regulates the supply of current to the heating coils. The resistances of the arms of the bridge can

be altered at pleasure; this gives a means of altering the temperature at which the galvanometer balances, and consequently the temperature of the chamber can be adjusted to whatever is desired.

A current is always passing through these heating coils and through a set of resistances in series with them; we will call these the "series coils." The less the resistance of the series coils, the greater is the current which passes through the heating coils; and if the series coils are short-circuited, the effect of their resistance is eliminated.



If the galvanometer swings over to the cold side, this short-circuiting takes place by the closing of a key, and more heat is supplied to the heating coils. This key is closed intermittently and remains closed for a short time. The amount of heat supplied to the heating coils can be increased in three ways:

1. The key can be closed more frequently.
2. The length of time during which it is closed can be increased.
3. The resistance of the series coils can be reduced.

The movement of the galvanometer boom regulates the supply of heat in all three ways. The first effect of a fall of temperature is that the short-circuit key is closed more frequently; this more frequent action of the key automatically increases the duration of each successive closure, and this again automatically diminishes the resistance of the series coils.

We will now describe the mechanism by which this is effected.

The suspended coil of the galvanometer carries a light horizontal boom of considerable length. A motor continually drives a horizontal axis to which is fixed a cam; this cam lifts up an arm (which we will call the hit-or-miss arm) capable of rotating about a horizontal axis, and then allows it to fall again. As the hit-or-miss arm falls, its end passes through an opening in a plate over which the galvanometer boom can pass; if the boom is on the "too hot" side, the end of the arm on its way down hits it, presses it down till it comes in contact with the plate, and thus the arm is prevented from being lowered to its full extent; if, on the other hand, the boom is on the "too cold" side, the arm is free to pass through the opening. In this case the arm is lowered still farther and carries with it the contact brush which acts as a key and completes contact, short-circuiting the series resistances. This, as before stated, increases the heat given out by the heating coils and raises the temperature of the chamber or vessel. The contact brush in its lowest position presses against a rotating cylinder; this gives a good rubbing contact, and as part of the cylinder is cut away, the brush falls quickly from its edge and the sparking is reduced. This cylinder rotates on the same axis as the cam and is fixed to it, and is thus driven by the motor. The cam is driven at a rate of about one turn per minute; at each rotation the arm is lowered, and the position of the galvanometer boom determines whether the contact brush makes contact or not. When the arm is in its upper position, the galvanometer is perfectly free to take up its position of equilibrium. The galvanometer is, in fact, a very delicate relay. There are stops which prevent the boom from moving too far in either direction; if the boom is pressing against one of these stops, there is always danger of its sticking there. This is a well-known difficulty, and when it happens, the galvanometer is not free to take up its position of equilibrium. To overcome this difficulty, the end of the hit-or-miss arm is made of such a shape that both in the case it hits, or in the case it misses, the galvanometer boom is moved away from the stops.

The cylindrical surface on which the contact brush rests is also capable of movement along the axis about which it rotates. The cylinder is cut away in such a shape that at one end of this endwise movement, the contact made by the brush is of very short duration,

and when at the other end the contact is comparatively long, and in intermediate positions the duration is intermediate between these extremes. It is this endwise movement which gives the second method of regulating the duration of the contacts, and consequently of the heat supplied to the heating coils. Every time the contact brush reaches its lowest position and contact is made, the cylinder is moved a small amount endwise, and the direction of this motion is such that the next contact will be of longer duration. Suppose the chamber is of too low a temperature, then contact will be made at every rotation of the cam, and the contact will get longer and longer each time, till at last too much heat will be entering the chamber, when the hit-or-miss arm is prevented from reaching its lowest position and no extra heat enters the chamber at that rotation of the cam. The converse then takes place; every time contact is not made the cylinder is moved endwise so that the duration of the contacts is successively reduced. It follows from this that if the hit-or-miss arm hits and misses the galvanometer boom alternately, the cylinder has no endwise displacement given to it in the long run; but if the hits and misses are not equal, endwise displacement will occur, and this goes on lengthening or shortening the duration of the contacts until there are as many hits as misses in a given interval of time. This endwise displacement is given by the same cam which moves the hit-or-miss arm up and down; the cam is simple in its action, but difficult to describe.

The third method of regulating the supply of heat is by altering the amount of the resistance in the series coils. This is done automatically by the endwise movement of the cylinder, which throws in or cuts out some of the series coil resistances.

In many cases this last arrangement will not be necessary, but in the spectroscope at the Cape of Good Hope the external temperature varied greatly, and consequently the quantity of heat to be supplied had also to vary greatly. The prisms might be only 5° F. hotter than the external temperature at midday, and they might be as much as 40° F. hotter than the external temperature at night. Thus at night eight times as much heat has to be supplied as during the day in order to keep the chamber at a constant temperature. Mr. Lunt, of the Cape of Good Hope Observatory, suggested this arrangement.

CAMBRIDGE, ENG.

THE ATLAS CHART FOR *T ORIONIS* EXTENDED.

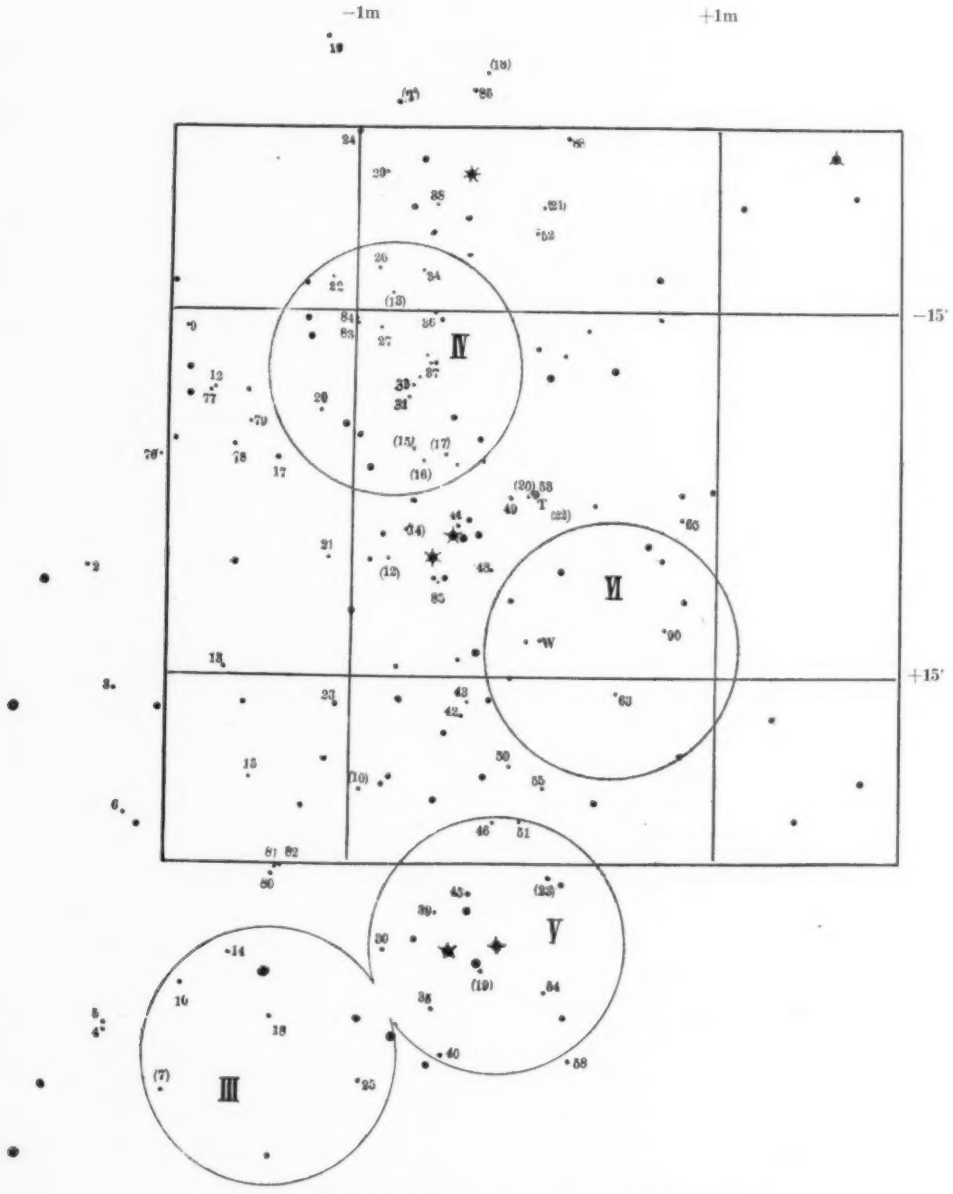
By JOHN G. HAGEN, S. J.

IN Volume **19** of this JOURNAL (pp. 344–349) a list of stars was published as a basis for a chart which might serve as a guide in observing the many variables recently discovered in the star cluster of the *Orion* nebula. The Atlas[1] Chart for *T Orionis* seemed to suit this purpose, if somewhat extended and filled in. This chart, however, is not supposed to make other more detailed charts unnecessary; it is only intended to locate the circular charts of Wolf, and to indicate the places of the variables given in these charts and in the Harvard *Circulars*. The positions of all the stars on the chart are published either in the Atlas Catalogue or in the list mentioned above, and its supplementary tables, which follow below. No attempt was made at inserting stars from available photographs by eye-estimates. The chart, therefore, will alone not suffice to identify all the new variables, but will serve as a guide in using the charts of Bond and of Wolf. The star disks of the new variables are relatively too large, but easily distinguishable by the appended Harvard numbers.

Since the publication of the Harvard *Circular* No. 78, two more *Circulars* have appeared, No. 79 and No. 86, with additional discoveries and measures of variables in the same region. In Table II of No. 86 there are eight new variables, which, with eight *B. D.* stars not yet contained in our first list, are tabulated in the supplementary tables below. We have thus on this chart $73+8=81$ variables, more or less confirmed.

The four circular charts of Wolf, which fall within this chart, are indicated by circles and marked III, IV, V, VI, according to Wolf's numbering. Instead of the numbers (6), (8), (9), (24), (25), which referred to five stars not yet confirmed as variable in *Circular* 78, the corresponding new numbers 76, 78, 80, 88, 90 of *Circular* 86 are printed on the chart. The star marked *W* is Wolf's b_3, and (*T*) denotes Bond 539, to which Pogson had applied the letter *T*.

[1] This refers to the author's *Atlas Stellarum Variabilium*, Berlin, 1899.—EDS.



EXTENSION OF HAGEN'S CHART FOR *T ORIONIS*.

The two stars Bond 642 and 654, which Mr. J. A. Parkhurst[1] has found to be variable, are too near the bright star θ^1 to be printed on the chart.

The three stars Nos. 5, 6, 8 of Table I in *Circular* No. 86, which were suspected by Isaac Roberts, are not inserted, as requiring further confirmation or identification.

SUPPLEMENTARY TABLES.

TABLE I.

P.	Chart	$\Delta\alpha$	$\Delta\delta$
77	....	$-1^m\ 49^s$	$-\ 8.''3$
79	....	$-1\ 36$	$-\ 5.6$
81	....	$-1\ 26$	$+31.0$
82	....	$-1\ 25$	$+31.0$
83	IV	$-1\ 1$	-13.7
84	IV	$-1\ 1$	-14.0
85	....	$-0\ 31$	$+\ 7.6$
86	....	$-0\ 23$	-33.0

TABLE II.

B. D.	Mag.	$\Delta\alpha$	$\Delta\delta$
$-5°1287$	9.3	$-2^m\ 58^s$	$+18.''5$
89	8.5	$-2\ 43$	$+\ 7.7$
91	9.5	$-2\ 11$	$+27.3$
92	9.3	$-2\ 4$	$+17.9$
93	9.8	$-2\ 0$	$-\ 4.0$
$-4°1164$	8.0	$-2\ 56$	$+54.1$
65	9.0	$-2\ 39$	$+50.2$
82	9.2	$-0\ 35$	$+46.6$

As in the previous table, the heading "P" refers to the numbers of Professor Pickering, and the heading "Chart" to Wolf's circular charts; the magnitudes are taken from the *B. D.* and the places are relative to *T Orionis* in the center of the orginal chart.

GEORGETOWN COLLEGE OBSERVATORY,
October 1904.

[1] ASTROPHYSICAL JOURNAL, **20**, 136–139, 1904.

Minor Contributions and Notes.

A DIVISION OF THE STARS IN SOME OF THE GLOBULAR STAR CLUSTERS, ACCORDING TO MAGNITUDE.[1]

The program of observations for the Crossley reflector undertaken by the late Professor Keeler contained the following eight well-known globular star clusters:

N. G. C.	R. A.	Decl.
5272	$13^h\ 38^m$	$+28°\ 53'$
5904	15 13	+ 2 27
6205	16 38	+36 30
6218	16 42	− 1 46
6656	18 30	−24 00
7078	21 25	+11 44
7089	21 28	− 1 16
7099	21 35	−23 38

An exposure of $1^h\ 30^m$ to 2^h was required to bring out the faint stars which, in telescopes of ordinary size, give the appearance of nebulosity to these clusters.

The general appearance of these clusters is very similar; they are of nearly the same angular dimensions, and the magnitudes of the component stars are remarkably alike.

In each of these clusters practically all of the stars can be separated into two classes of magnitudes. Perhaps a third of the whole number lie between the eleventh and thirteenth photographic magnitudes, while almost all of the remainder are very faint, being about the sixteenth magnitude. The appearance is that of two layers, one of bright stars superposed upon another of very faint stars.

In his study of the *Hercules* cluster (*M* 13), Palmer[2] notes this division of magnitudes and gives the number of "bright" stars in that cluster as 1016, and of "faint" stars as 4466.

No numerical magnitudes are assigned, owing, probably, to the difficulty of determining magnitudes with sufficient accuracy from the Crossley

[1] Also to appear in a *Bulletin* of the Lick Observatory.

[2] "The Distribution of Stars in the Cluster *Messier* 13, in *Hercules*," Astrophysical Journal, **10**, 246, 1899.

photographs. Scheiner[1] gives the magnitudes of the 833 stars in this cluster catalogued by him as from 12 to 14.

From visual observations of the cluster *Messier* 13 with the thirty-six-inch refractor, I should assign, for the brighter stars, a range of about 2 magnitudes, from 11 to 13, and of one magnitude, $15\frac{1}{2}$ to $16\frac{1}{2}$, for the faint stars. There is little departure from these ranges for the other clusters. But few stars of magnitudes 14 to $15\frac{1}{2}$ are to be found in these clusters.

Photographs of the ω *Centauri* cluster, obtained at the Harvard College Observatory station at Arequipa, exhibit the same division of the stars in that cluster into two groups as does the Crossley series. The limits of the area of the faint stars are fully as sharply defined as those of the brighter ones and the centers of the two groups coincide. We are, therefore, led to the conclusion that, in each case, the observed division is in the same group of stars in space. That it is the characteristic of all clusters of this type is not certain, but the lack of any exceptions in the clusters observed suggests such a hypothesis.

The clusters under consideration are widely distributed, covering eight hours of right ascension and over seventy degrees of declination. Only one of them (*N. G. C.* 6656) is in the Milky Way.

In the case of the *Hercules* cluster, Palmer found that the faint stars appeared to be distributed, approximately uniformly, in a sphere having a nine-minute radius, whereas the bright stars are more numerous near the center of the cluster. This result for one cluster, coupled with the general appearance of all clusters, leads to the belief that the real form of these objects (at least the nine in question) is spherical.

Two hypotheses, to account for the peculiar distribution in magnitude, suggest themselves:

1. That it is due to a difference in the size of the stars.
2. That it is due to a difference in constitution or physical condition.

The almost complete lack of physical data at present prevents any useful discussion of these hypotheses. Although the first appears to be the more probable, yet it is conceivable that this peculiar distribution might result from a difference in constitution of the stars themselves.

The suggestion has been made that an absorbing medium pervades the cluster. It is difficult to see how such an appearance as that noted could result from absorption of the light, upon any reasonable assumption as to the character and distribution of such a medium. The result of such absorption, in a cluster of globular form, should cause a general diminution

[1] "Der Grosse Sternhaufen im Hercules *Messier* 13," *Abhandlungen der k. Akademie der Wissenschaften zu Berlin*, 1892.

in the brightness of the stars as we approach the center of the cluster. No such effect is to be detected, however.

In this connection, attention may be called to the relation which has been supposed to exist between the nebulæ and the star clusters. The belief has been gaining way, since photography has shown the real structure of so many of those objects, that they are but different stages in the process of evolution; that a star cluster has been formed by the condensation of the matter in the nebula.

A study of the nebulæ which have been observed with the Crossley reflector shows that a large proportion are spiral, and that practically all the spirals are lenticular or disk-shaped. Many of them are relatively very thin. Now, if the globular clusters are really spherical, as seems probable, it is difficult to see how they could have originated from a disk-shaped nebula (spiral?).

As there are other forms also of nebulæ and star clusters, it is not necessary to assume that the order of evolution mentioned above is the only one. But the natural tendency has been to connect such changes as these with the spirals which, by their appearance, seem to indicate greater systematic internal activity than others.

C. D. PERRINE.

MOUNT HAMILTON, CALIFORNIA,
July 17, 1904.

THE NUMBER OF THE NEBULÆ.[1]

PROFESSOR KEELER, soon after beginning his program of work with the Crossley reflector, showed that the number of nebulæ is very much greater than had been supposed. He conservatively placed the number within reach of that telescope at 120,000.

His program comprised the taking of photographs of 104 of the brighter nebulæ and clusters located in all parts of the sky within reach of the telescope, *i. e.*, north of declination $-25°$. The recent completion of this program enables us to revise his estimate.

In 57 of the regions 745 *new nebulæ* have been discovered. Almost all of them are very small and faint. The regions in which no new ones were found were, as a rule, those surrounding the clusters and very large nebulæ. There were 142 known nebulæ observed, in these regions, making the total number observed 887, an average of $8\frac{1}{2}$ per region. As it would take 62,000 such photographs to cover the entire sky, the results indicate 500,000 as the corresponding number of nebulæ within reach of the Crossley

[1] Also to appear in a *Bulletin* of the Lick Observatory.

reflector. This assumes that the small portion observed represents fairly the entire sky. It is well known that the nebulæ are much more numerous in some parts of the sky than in others. This is a tendency which, so far as we know, affects large and small nebulæ alike.

The fact that a considerable number of other subjects than the nebulæ (presumably non-nebulous regions) are included in the program, indicates that the portion observed is fairly representative of the whole sky.

Longer exposures, more sensitive plates, and more perfect photographs will undoubtedly reveal some nebulæ which do not now appear, and others which are confused with the faint stars. It seems probable, therefore, that the number of the nebulæ will ultimately be found to exceed a million.

The positions of the new nebulæ discovered on the Crossley photographs have been determined, and a catalogue of them will be printed in the volume of reproductions of nebulæ and star clusters, soon to be issued.

C. D. PERRINE.

MOUNT HAMILTON, CALIFORNIA,
June 18, 1904.

THE NINTH SATELLITE OF *SATURN*.[1]

IT is probable that in the future there will be no difficulty in securing a sufficient number of observations of *Phoebe*, the Ninth Satellite of *Saturn*, not only to correct the present elements, but to study the large and interesting perturbations to which it is subject. It can be observed visually with the largest refractors, and can doubtless be photographed with large reflectors, as well as with the Bruce telescope, by the aid of which Professor William H. Pickering discovered it. Since the observations enumerated by him in the Harvard *Annals*, **53**, 55, 60, *Phoebe* has been closely followed by Professor Bailey. The approximate positions obtained by him with the Bruce telescope are given in Table I, and the positions found by Professor Barnard visually on August 8 and September 12, 1904, with the

TABLE I.

Positions of *Phoebe*.

Desig.	Date	G. M. T.	Exp.	x	y	Desig.	Date	G. M. T.	Exp.	x	y
	1904	h m	m	′	′		1904	h m	m	′	′
A 6771	June 18	18 20	120	+24.20	+6.65	A 6844	Aug. 4	14 44	..	+10.86	+1.61
A 6773	20	18 30	120	+23.84	+6.65	A 6846	5	14 32	..	+10.60	+1.37
A 6801	July 6	16 15	120	.....	.....	.....	8	18 0	..	+ 9.70	+0.98
A 6804	7	16 14	120	.....	.....	A 6854	15	19 15	..	+ 6.83	+0.15
A 6807	11	16 58	180	.....	.....	A 6856	16	18 51	..	+ 6.40	0.00
A 6841	Aug. 2	14 47	...	+11.56	+1.85	.....	Sept. 12	12 36	..	+ 5.93	−4.28

[1] *Harvard College Observatory Circular* No. 87.

forty-inch Yerkes telescope, and announced in the Harvard *Bulletins* Nos. 157 and 159, are added, to bring together all the material so far collected. The designation of the plate, the date, Greenwich Mean Time, exposure, and rectangular co-ordinates referred to *Saturn* as an origin, are given in the successive columns.

For some unexplained reason, *Phoebe* has not been found on the plates taken in July. The record for the plates taken in August, has not yet been received from Arequipa.

A NEW VARIABLE IN *HERCULES*.

The meridian photometer, like other meridian instruments, is not adapted to the discovery of variable stars. It may therefore be of interest to note the discovery of such an object by the writer, with the twelve-inch Meridian Photometer. On August 23, 1904, while measuring the star $+24^{\circ}.3419$, mag. 9.4, it was noticed that a brighter star, having the photometric magnitude 9.5, and not in the Bonn Durchmusterung, preceded it. An examination, the next day, of the photographs of this region at once showed that the star was a variable of long period having a range extending at least from the magnitude 9.5 to <13. The approximate position for 1855, is R.A., $18^{h}\ 20^{m}\ 26^{s}.0$; Dec., $+24^{\circ}\ 56'.4$.

EDWARD C. PICKERING.

SEPTEMBER 12, 1904.

Reviews

The Moon: A Summary of the Existing Knowledge of Our Satellite, With a Complete Photographic Atlas. By William H. Pickering, of Harvard College Observatory. Pp. viii + 103, with 100 illustrations. New York: Doubleday, Page & Co., 1903. Price $10.

It is a fact that the Moon has been badly neglected visually, in recent years, and that its study has been relegated to the amateur with small instrumental means. The large telescopes of today have never seriously taken up its study. Yet there is perhaps no object in the sky that would more probably repay the careful observer than a close study of the Moon's surface, but this evidently must be a study of details, because only in such is it likely that any discoveries of importance would be made. We have perhaps come too early to the conclusion that the Moon is a dead world and that no real changes take place upon its surface. But in the light of the powerful telescopes of today this might be a very rash conclusion. That its study has not been intelligently taken up with modern telescopes is perhaps due to the fact that, for one thing, it appears too easy and too commonplace. Another reason possibly may be that its great brightness would be injurious to the eyes of an observer who wished also to observe other and especially faint objects. This, coupled with the tradition that it was dead and that no changes took place upon its surface, perhaps produced the foregone conclusion that it would be a waste of time to take up the study of the Moon seriously.

Whatever the explanation of this neglect may be, it is true that observers in general had evidently come to the conclusion that the Moon was an object fit only to show to the necessary visitor, or to be execrated for spoiling the night with its undesired brilliancy as the Sun had already partly done by cutting out a large portion with which to make the day.

The Moon, therefore, it would seem, might offer a rich field for careful and original investigation with sufficiently powerful telescopic means.

Professor William H. Pickering has taken up this subject in recent years, and his results are so startling—bordering, as they do, on the sensational—that one hesitates to accept them, or rather to accept his conclusions.

He claims to have found evidences, not only of present volcanic life, but of snow and ice, clouds and vegetation.

Perhaps if these discoveries had come slowly and one at a time, with long intervals between, they might have been received with better grace; but they have been turned out by wholesale, and almost any place on the Moon would seem to be conspicuously productive of one or more of the above phenomena; even the fateful canals of *Mars* are found to be denizens of the Moon also.

Professor Pickering's results have been published in the large quarto volume which has been placed in my hands for review.

The book is conveniently separated into various chapters dealing with the origin of the Moon, in which the researches of George Darwin are popularly retold: the reasons for believing the Moon's atmosphere must be very attenuated—its small mass making it impossible for the Moon to prevent the escape from it of the gases necessary to make an atmosphere; the origin of the lunar craters, in which he shows that many of the theories that have been offered to account for them are untenable—such as the impact of huge meteorites when the Moon was in a plastic condition, etc. He finally comes to the conclusion that there is a strong resemblance in many of the volcanic features of the Hawaian islands to lunar phenomena, and concludes that the lunar craters have been formed by processes not materially different from those on the Earth, except where vaster results have come from the smaller mass of the Moon.

One chapter treats of vegetation and the lunar canals. The author concludes that, though a certain scant vegetation exists on the Moon, no intelligent life can exist there, and indirectly proves that the canals on *Mars* cannot be due to the work of man.

> Sixthly, since no water in the liquid form can exist on the Moon, this fact will enable us to rule out many seductive but erroneous hypotheses. Seventh, and lastly, we know that so little air and water vapor exist there that we can confidently also rule out all aid in the construction of these formations [the "canals"] from intelligent or intellectual life . . . . they merely weaken the strongest argument hitherto found for the existence of highly intelligent life upon *Mars*.

In this chapter Professor Pickering gives comparisons, side by side, of drawings and photographs of certain craters to prove the existence of the lunar canals. The photographs used in this comparison are so excessively enlarged that the details are mere blotches and it is hard to say what they represent. In these pictures, plates E and F, the resemblance between the drawings and the photographs are certainly very vague, and an interested mind could possibly trace out the canals or any other desired feature, but to the unimpassioned mind the various canals shown in the drawings have to be imagined on the photographs.

A chapter devoted to the formation of artificial craters gives many interesting experiments in which lunar-like formations are produced. Perhaps by such experiments some idea may be obtained of the possible formation of the lunar craters. There is danger, however, of coming too readily to conclusions that may be wrong, from the very fact that any experiments must be on such a minute scale, compared with the forces that have been at work on the Moon, that a comparison might be entirely misleading.

The author concludes, however, that the *maria* were never the abode of bodies of water, and that they are not, therefore, the beds of former oceans, as some have supposed; and also that their formation was perhaps due to the solidification of great lakes of molten matter after the surrounding regions had solidified.

In reference to the ice and snow, which Professor Pickering finds on the Moon, there seem to be large regions covered with this so-called ice and snow. In Plate C he gives two reproductions from the same full Moon negative in which one is printed out until only the very brightest regions remain. By this method Professor Pickering endeavors to separate the "snow"-covered regions from the ordinary bright, so-called volcanic regions. By this heavy printing there are left regions of extra brightness, while the generally bright regions have nearly all disappeared, leaving the snow areas alone visible. Is not this a trick of photography itself in which the very brightest regions are differentiated from the rest by excessive overprinting? Might not this as readily be carried farther and still brighter regions alone be left, and, if so, what would they be called? How these residual bright regions thus revealed can be called "snow" does not yet seem quite clear. It would appear to be simply a survival of the brightest.

Speaking of the bright rays from Tycho, Professor Pickering says:

> What has hitherto been considered one of the strangest features is that they are never visible at lunar sunrise or sunset, but require that the Sun shall have an altitude of at least 5° or 10° in order to render them visible. This peculiarity we have already explained as being due simply to the fact that the snow which forms them lies in crevices instead of on a smooth surface. The Sun must therefore necessarily attain a certain altitude before they can become visible.

But at such altitudes of the Sun these supposed crevices should themselves become visible from the shadows within them. As they are not so visible, it will be necessary to find some other means of hiding the snow near the time of sunrise.

Professor Pickering instances the crater Linné as perhaps the best evidence of changes still taking place on the lunar surface. The history

of this object is well known. The earlier astronomers designate it as a small crater of considerable size and depth. Since Schmidt's observations of 1866, when it seemed to have disappeared to him, it has been a small and rather difficult crater, and must have been beyond the reach of some of the earliest telescopes that described it. It is not now one-sixth the size formerly attributed to it. After the Sun has risen upon Linné for a few days, the crater-like formation gives place to a rather large nebulous spot of light, several times the size of the real crater. This luminous spot, Professor Pickering finds, becomes smaller as the Sun rises higher upon it. He attributes this appearance to a deposit of hoar frost about the crater during the lunar night (in some way due to the activity of the crater) and the subsequent melting of it by the Sun. If this is frost so formed, it is not quite clear why it should not be visible when the Sun first shines upon it. Is it not as probable that some peculiarity of the immediate surface surrounding the crater, along with the aid of the Sun's light at certain angles, is responsible for the phenomena? A more conclusive proof of Professor Pickering's idea would be from observations of this object immediately before and after an eclipse of the Moon. Such observations he has made. Measures of the diameter of this spot of light were obtained by him at the eclipse of December 16, 1899. His measures showed that the spot increased 0".14 in diameter through its immersion for some two and a half hours in the shadow of the Earth. This spot is, however, so excessively ill-defined in a telescope that the measures of its diameter may well differ by 1" of arc. Therefore it would be an even chance that a difference several times his value would be bigger or smaller in the measures of its diameter on such an occasion. But at the eclipse of October 16, 1902, Professor Pickering found an increase in size apparently due to additional accumulations about the crater while in the shadow of 2".8, which is a very measurable quantity in even a vague spot like that around Linné. There is no question but that this spot does apparently decrease in size with the increase of the Sun's altitude and afterward increase as the Sun goes down, as Professor Pickering has claimed for it.

Professor Pickering finds numerous other cases of change which he believes to be due to present volcanic activity. These observations are so startling that until they are fully verified one hesitates to accept them as real. Some of these phenomena consist of shifting white streaks or areas which he attributes to "streams of gas issuing from the craters and carrying with them white crystals of snow, thus forming real clouds upon the Moon."

The author's accounts of the origin of the Moon, its motions, phases, etc.; why one face of the Moon is always turned toward us; the probability

of an atmosphere, and the reasons he gives for believing such an atmosphere to be very thin, etc., are contained in chapters 1, 2, and 3. They are interesting reading and are clearly written, and must be of value to the ordinary reader.

Chapter 10 is also interesting as giving an account of the various superstitions, etc., connected with the Moon.

Some of the other chapters contain matter that would be interesting to the general reader, but where the author has gone into minute details of his own observations and conclusions, with frequent reference to the photographs, the ordinary reader would find it difficult to follow him.

The book is really the result of an expedition undertaken by Professor Pickering in 1900 and 1901 to the island of Jamaica, in latitude +18°, with the special object of studying and charting the Moon with a telescope 12 inches in diameter and of 135 feet 4 inches focus, which gave a 16-inch image of the Moon.

This instrument was mounted on the slope of a hill so that the axis of the telescope pointed to the north pole of the heavens, the lens remaining stationary. The light from the Moon was thrown into the object-glass by a movable flat mirror 18 inches in diameter, which was made to revolve westward at the rate of one revolution in twenty-four hours.

After experimenting with the instrument, it was found best to reduce the aperture to 6 inches, and with this essentially all the photographs were made. This extremely great ratio of aperture to focus—1:270—was necessarily very slow, besides interfering seriously with the separating power of the instrument. Even with the bright Moon the exposures in some cases were of two minutes' duration.

With this telescope eighty photographs were obtained (during the seven months' work) which covered the Moon's surface completely five times. These photographs, which are bound in the last part of the volume, are reproduced of the original size, only a portion of the Moon's surface, however, being shown on each plate. There are five plates of each region, and these are under different illuminations.

A study of these same regions under different illuminations is highly interesting and instructive. They show most strikingly the effect of varying illumination on craters and streak systems. Especially interesting are the full Moon phases.

The definition is seldom good in these photographs. Doubtless this is due to unsteadiness during the long exposures made necessary by the relatively small aperture. Though this seriously detracts from a close study of details, yet it does not materially affect their study as a chart and for the examination of the general features.

Following these are the real charts—by quadrants. These are on a large and convenient scale. On them are drawn lines of parallels and meridians. For each of these four pictures is a corresponding hand-drawn chart on which are printed the names of the different objects, from which they are easily recognized on the photographs. Though these four photographs are lacking in definition and have had, for the sake of exactness, the principal objects outlined by hand, they are, nevertheless, of great value for the location and study of details on better photographs. An accompanying table gives the approximate longitudes and latitudes of the various objects, with their names.

For convenience in comparison with the text, it would have been better to have bound the photographs and charts separately from the text.

The following list of errata has been printed by the publishers:

ERRATA

Page 32, line 31, for 5.7 read 5.6.

Page 37, line 18, for (64,159) read (2.5, 6.3).

Page 37, line 25, Plate 3B is inverted. These measures should therefore be made from the left and top.

Page 37, line 29, for 6.3 read 6.2.

Page 37, line 31, for 6.6 read 6.5.

Page 39, line 5, for 4.1 read 4.2.

Page 43, line 16, for 8 read δ.

Page 49, line 19, for B read C.

Page 49, line 25, for Messier, A read Messier A.

Page 52, line 2, see page 37, line 25, above

Plate F. The straight dark lines on Figs. 6 and 7 are defects. Fig. 7 should be turned one-quarter way around, so as to resemble Fig. 8.

Page 68, line 18, for 33 read 13.

Page 70, line 27, for 3B (1.2, 8.5) and 3E (1.1, 8.6) read 2A (3.2, 1.2) and 2C (3.2, 1.8).

Page 70, line 28, for 5 and 8 read 3 and 6.

Plate H, last line, for 22 read 2.

Page 103, line 1, for contents read constants.

Plate 3B. This plate is inverted.

E. E. B.

Observations of Variable Stars Made at the Rousdon Observatory, Lyme Regis, under the Direction of the late Sir C. E. PEEK. Edited by H. H. TURNER. *Memoirs of the Royal Astronomical Society*, Vol. **55**, 1904.

THIS valuable memoir contains about five thousand observations of twenty-two long-period variable stars, mostly northern circumpolar, made

between 1887 and 1901. They have been carefully edited and thoroughly discussed by Professor Turner, and the resulting volume is one of the most complete that have ever appeared in this branch of astronomy, and may well serve as a model for future publications of variable star work.

The series of observations is remarkable in several respects, notably for its homogeneity, its continuity, and its inclusion of (nearly) complete light-curves of the variables. The stars observed were:

T Cassiopeiae	*S Ursae Majoris*	*T Draconis*
S Cassiopeiae	*S Boötis*	*R Cygni*
S Persei	*R Camelopardalis*	*χ Cygni*
R Aurigae	*S Coronae*	*S Cygni*
U Orionis	*R Ursae Minoris*	*T Cephei*
R Lyncis	*R Draconis*	*S Cephei*
R Ursae Majoris	*S Herculis*	*R Cassiopeiae*
T Ursae Majoris		

As far as the aperture of the telescope (a Merz refractor of 6.4-inch objective) permitted, the variables were followed throughout their cycles, which gives great value to the series. The observations were all made by C. Grover, assistant at the private observatory. The methods of observation were notable in two respects: The variable was usually compared with five neighboring stars, therefore large magnitude intervals were often used, frequently more than a magnitude, and sometimes two, three, or even four magnitudes. Evidently the method cannot be called "Argelander's," and it is a question whether the large intervals were not better omitted in cases where at least two comparison stars nearly equal to the variable were included. The hour of observation was not recorded, so that difficulty was met in tracing the effect of varying hour angle on the estimates.

In regard to the appearance of the stars, the following quotation from the author's introduction is interesting:

> Great attention has been devoted to the visual physical appearance presented by these variables. It was soon found that they differ in a remarkable degree from ordinary stars. Most of them are of a deep red or ruddy color, and many are more or less nebulous. They may be divided into four classes, viz., stars having—
>
> *a*) A remarkably well-defined, almost planetary disk
>
> *b*) Well-defined stars surrounded by a more or less dense, ruddy atmosphere.
>
> *c*) Large woolly stars, with ill-defined image, resembling a small but bright planetary nebula.
>
> *d*) Stars which at minimum show, in place of the variable, a slight bluish nebulosity.

The reviewer has observed fifteen of these stars with telescopes ranging from six to forty inches in aperture, but has been unable to confirm this nebulous appearance, either with the twelve-inch Brashear or the forty-inch Clark refractors, or by visual or photographic observations with the six-inch or twenty-four-inch reflectors. How much of this appearance may have been due to the Merz objective is a question.

The reductions and discussions by Professor Turner are worthy of the highest praise. The limits of this review permit the mention of only a few salient points:

1. The magnitudes of the comparison stars were based on the photometric values given in *Annals of the Harvard College Observatory*, Vol. **37**, but so combined with the results of the Rousdon comparisons as to represent that work better, while keeping the general Harvard scale. This permits a direct comparison with the simultaneous Harvard observations of the same stars; perhaps the most important fruit so far borne by Professor Pickering's efforts toward co-operation in this field.

2. To give a "bird's-eye" view of the results, the mean light-curve for each variable is formed, and the deviations of the separate curves are given in tabular form—a very compact and satisfactory method, showing at a glance the star's behavior and permitting a deduction of the correction to the assumed period.

3. Light on the quality of the observations is therefore thrown from two sources—the deviations from the mean curve and the comparison with the simultaneous Harvard observations. Curious results follow: first, the deviations from the mean curve are disappointingly large, frequently more than a full magnitude both in the Rousdon and Harvard work; second, the large deviations in the one series are not usually confirmed by the other; thus the discordances between the series are still larger, amounting at times to more than a magnitude for slightly colored stars and more than two magnitudes for the deep red stars. One is at a loss to account for such large discordances (similar though shorter series known to the reviewer differ by three tenths of a magnitude at most), but they lead the editor to the weighty conclusion:

4. "We are thus led to the important conclusion that the apparent deviations from a mean curve are due to the observer and not to the star; or, in other words, the variations of brightness in the variable are perfectly regular, though the difficulties of observing introduce apparent deviations." The editor is thus led to undertake a preliminary harmonic analysis of the light-curves, but, though the results are promising, he concludes: "We are in face of a problem of some complexity, which will require the careful

discussion of all the material available, and not merely of one set of observations; and such a discussion cannot be entered upon here."

5. Two things strongly urged by the editor for future work are: first, the publication of detailed comparisons of the variables; second, the publication of mean light-curves. A careful study of the foregoing work fully justifies these recommendations.

P.

NOTICE.

The scope of the ASTROPHYSICAL JOURNAL includes all investigations of radiant energy, whether conducted in the observatory or in the laboratory. The subjects to which special attention will be given are photographic and visual observations of the heavenly bodies (other than those pertaining to "astronomy of position"); spectroscopic, photometric, bolometric, and radiometric work of all kinds; descriptions of instruments and apparatus used in such investigations; and theoretical papers bearing on any of these subjects.

In the department of *Minor Contributions and Notes* subjects may be discussed which belong to other closely related fields of investigation.

Articles written in any language will be accepted for publication, but unless a wish to the contrary is expressed by the author, they will be translated into English. Tables of wave-lengths will be printed with the short wave-lengths at the top, and maps of spectra with the red end on the right, unless the author requests that the reverse procedure be followed.

Authors are particularly requested to employ uniformly the metric units of length and mass; the English equivalents may be added if desired.

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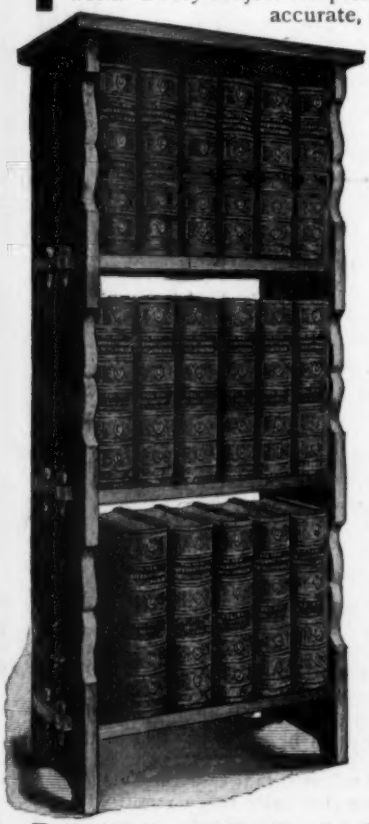

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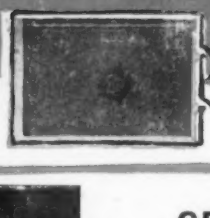

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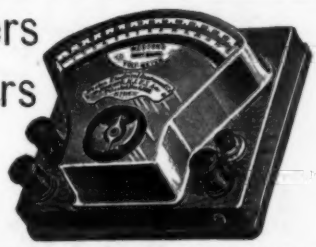

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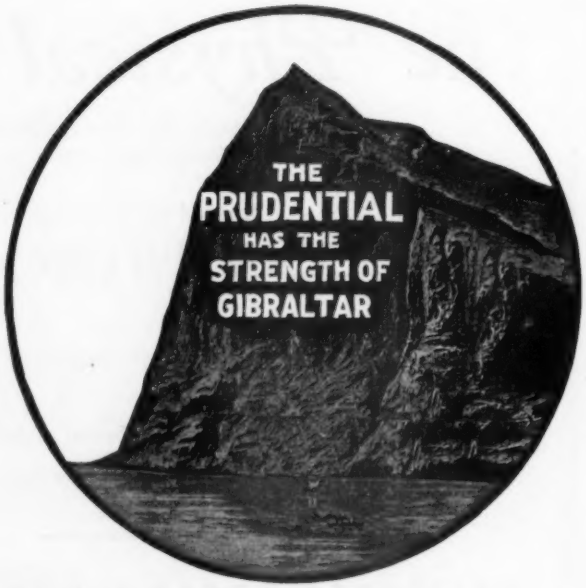

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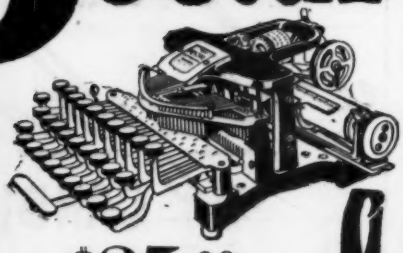

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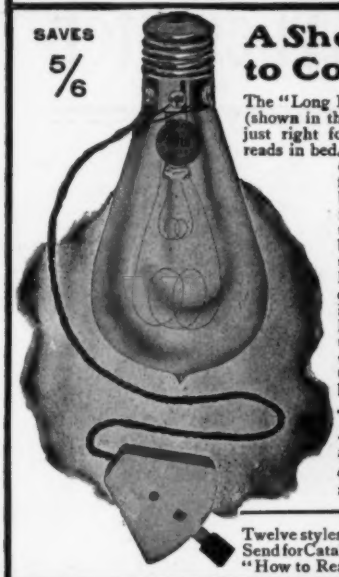

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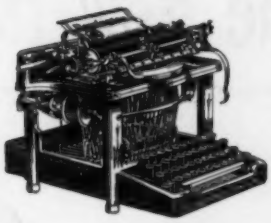

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